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THE SYSTEM OF THE WORLD
CHALLENGE LECTURES

W. L. JORDAN

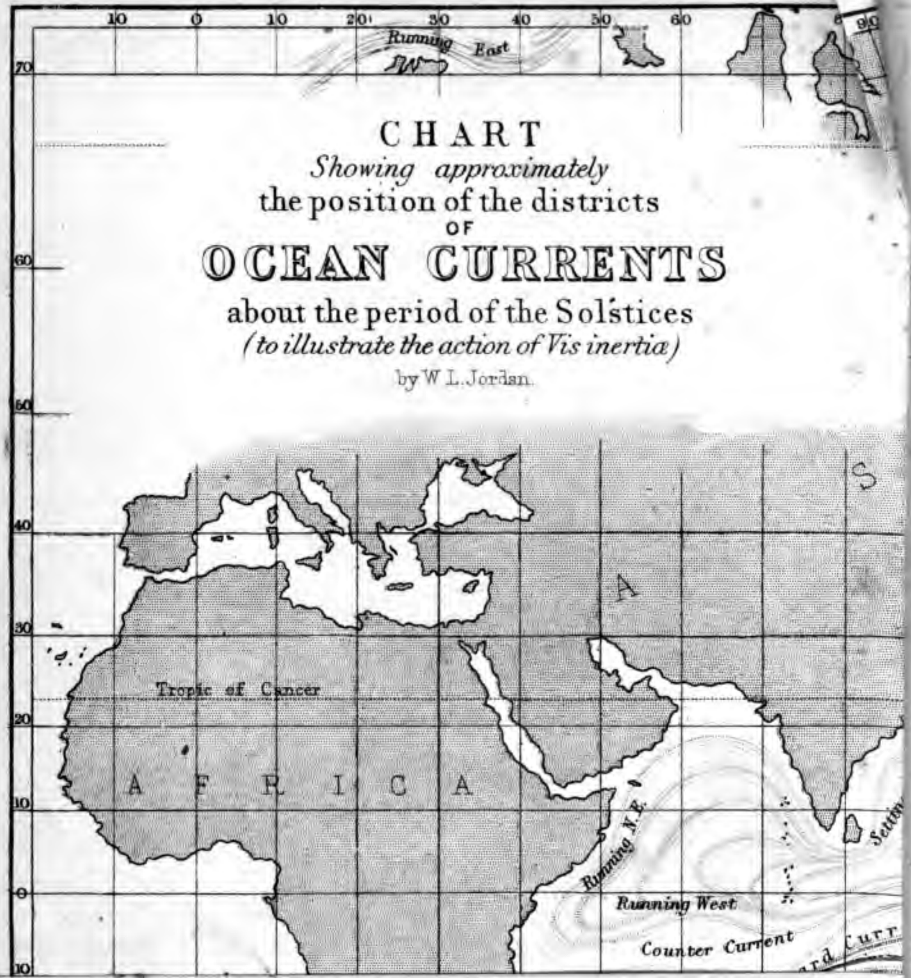
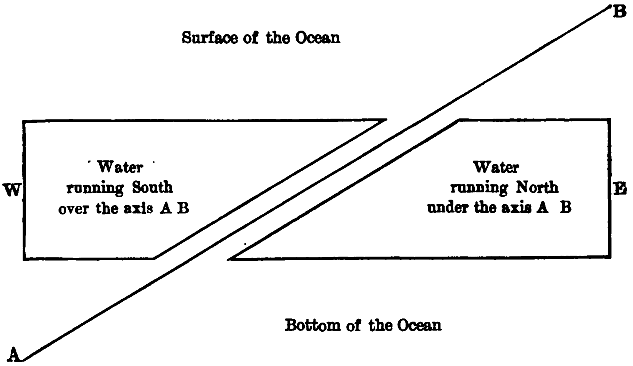


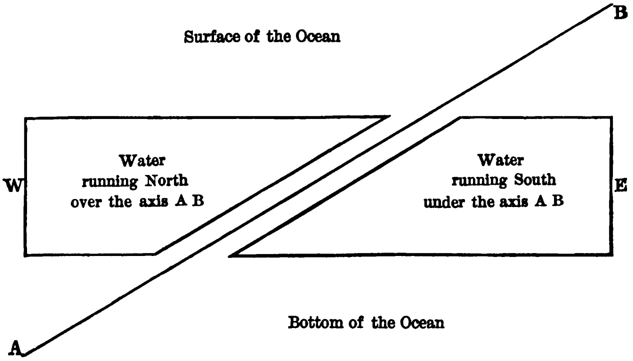
CHART
Showing approximately
the position of the districts
OF
OCEAN CURRENTS
about the period of the Solstices
(to illustrate the action of Vis inertia)
by W. L. Jordan.

PLATE II.

Perpendicular Sections of the Districts shown on Plate I.



SECTION OF ARCTIC AND SOUTH EQUATORIAL DISTRICTS, COLOURED BLUE IN THE MAP.

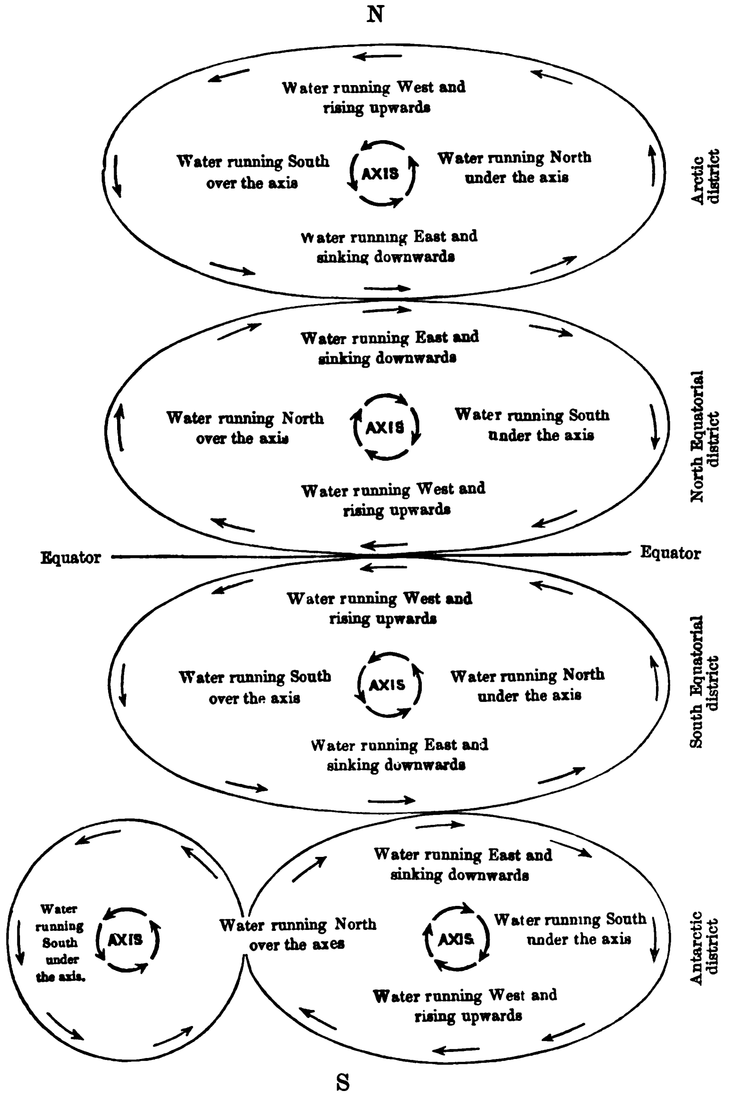


SECTION OF NORTH EQUATORIAL AND ANTARCTIC DISTRICTS, COLOURED RED IN THE MAP.

The two lesser Antarctic districts, coloured green in the map, each have their axis inclined in the opposite direction to the above; but the currents running north pass over the axis, and those running south pass under the axis, just as in the other Antarctic districts.

PLATE III.

Horizontal Section, applicable to either the Atlantic or Pacific Division of Currents shown in Plate I., taken half-way between the Surface and the Bottom of the Ocean.


N
Water running West and rising upwards
Water running South over the axis
AXIS
Water running North under the axis
Water running East and sinking downwards
Arctic district
Water running East and sinking downwards
Water running North over the axis
AXIS
Water running South under the axis
Water running West and rising upwards
North Equatorial district
Equator
Equator
Water running West and rising upwards
Water running South over the axis
AXIS
Water running North under the axis
Water running East and sinking downwards
South Equatorial district
Water running South under the axis.
AXIS
Water running East and sinking downwards
Water running North over the axes
AXIS
Water running South under the axis
Water running West and rising upwards
Antarctic district
S

A LECTURE

ON THE

WINDS, OCEAN CURRENTS, AND TIDES

AND

WHAT THEY TELL OF THE SYSTEM OF THE WORLD.

DELIVERED AT WILLIS'S ROOMS, KING STREET, ST JAMES'S

ON SATURDAY EVENING, NOV. 3, 1877

BY

WILLIAM LEIGHTON JORDAN, ESQ., F.R.G.S.

SECOND EDITION

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1877

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THE Lecture reported in the following pages will be continued at 8 o'clock on the evening of Thursday, the 20th December, at Willis's Rooms.

Seats *5s.*, *2s. 6d.*, and *1s.*

For Tickets apply by letter to W. Leighton Jordan, Esq., the Scientific Club, 7 Savile Row, W.; or at Mr. Mitchell's Royal Library, No. 33 Old Bond Street, W.

PREFACE.

DEDICATED BY PERMISSION

TO

DR. WILLIAM HENRY HADEN-HADEN,

M.A., M.D., D.C.L., ETC.

◆

A FRIEND has, since the issue of this second edition, suggested to me the expediency of striking out from the following pages the remarks I made on the 3rd of November regarding the circulation of the blood, on the supposition that to some it will form a stumbling block in the way of accepting the more practical portion of the views I have expressed, whilst to others it may form a convenient handle for purposes of hostile criticism.

In view of this, I desire to emphasise what I stated on the 3rd November, to the effect that those remarks did not form an intrinsic part of the Lecture, and that I do not pretend in the following pages to have brought any argument to bear on that subject. In fact, when I rose to deliver the Lecture I had not the slightest intention of saying anything whatever on the subject. Having, however, said thus much, I am also bound to state that the opinion expressed is not one hastily or lightly formed. It is not based on directly inductive reasoning, but on *à priori* knowledge of the fact that the newly discovered action of gravitation must be in play affecting the living system, even if it were quite impossible for us to form any conception of the manner in which its action might produce effects. This latter is, however, far from being the case.

In the formation of a drop of water on a pane of glass, or on a blade of grass, we see one half of the requisite action. There the gravitation of the particles of moisture to each other causes them to rush together and so to form one large drop; and if we imagine this drop to be formed in an elastic covering, then, as fast as this covering is expanded by the inrush of the fluid particles, its resisting or contractile effort must tend to drive back the fluid; then, if there be channels connected with that elastic covering provided with valves, which in some of the channels, as in the veins, allow the fluid to pass into the elastic receptacle and not to flow back, then the contractile effort must drive the fluid out through those channels which, like the arteries, are not thus provided with valves to prevent such an exit, as fast as the tendency of the particles to gravitate together into one mass expands the elastic receptacle.

The remarks I made are not, however, based on any ideas of this sort, but on what I consider the discovery of the fact that gravitation is an act of *vis inertiæ*, and is the primary force which emanates universally from matter, and is in constant process of transmutation into other forces, all of which must of necessity be traced to it, or else be in direct conflict with it; so that every physical phenomenon is directly or indirectly connected with it. This forms an *à priori* reason for asserting that gravitation is in action on the circulation of the blood, even if it were impossible to imagine how it can act.

Vis inertiæ, which, by the action of gravitation, is constantly drawing the blood to the heart, is in incessant conflict with the vital force, whose contractile power as incessantly drives the blood out through the arteries. When the vital power is exhausted, then at length *vis inertiæ* is no longer resisted, and its last action of gravitation, which draws the contents of the veins into the heart, retains the blood there in the absence of sufficient vital force to resist this aggregation of the fluid particles caused by the direct action of gravitation. The arteries are empty when the force of gravitation which draws the blood to the heart is no longer exceeded by the vital force which drives the blood from the heart through the arteries. The fact that in cases of starvation or hæmorrhage the heart is empty and contracted tends to confirm the foregoing view, because, in the one case the last drop of blood has been absorbed by the system, and in the other case it has been removed ; so that in each case there is none left free to gravitate towards the heart.

INTRODUCTION.

THE following advertisement appeared in several of the London daily and weekly papers at intervals preceding the Lecture :—

'A Lecture on "The Winds, Ocean Currents, and Tides, and what they tell of the System of the World," will be delivered at Willis's Rooms, King Street, St. James's, on Saturday evening, November 3rd, 1877, by William Leighton Jordan, Esq., F.R.G.S., author of

"The Elements: an Investigation of the Forces which determine the Position and Movements of the Ocean and Atmosphere" (1866);

"Vis-inertiæ: a Treatise on the action of Vis-inertiæ in the Ocean" (1868);

"The Ocean: its Tides and Currents, and their Causes" (1873);

"Remarks on the recent Oceanic Explorations" (1877);

"The Winds, and their Story of the World" (1877).[1]

The principal basis of the Lecture will be a Chart and Sections of Oceanic Circulation published with the first of the above-mentioned works in 1866.[2] The attendance of Fellows of the Royal, Royal Astronomical, and Royal Geographical Societies is earnestly requested.

Members or Ex-Members of the Council of the Royal Society and the Scientific Staff of the "Challenger" Expedition are specially invited to discuss what will be said by the Lecturer in demonstration of the new theory of Vis-inertiæ, or the conflicting action of Astral and Terrestrial Gravitation.'

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[1] Published in April.

[2] This Chart and Sections form the frontispiece to this pamphlet.

LECTURE.

LADIES AND GENTLEMEN,—

I do not to-night pretend to offer you a theoretical demonstration of the new theory of Vis-inertiæ. I invite your attention to a sketch of what that theory is, and then I hope to give you a practical demonstration that it has been proved to be true by investigations which have been made since the theory was published.

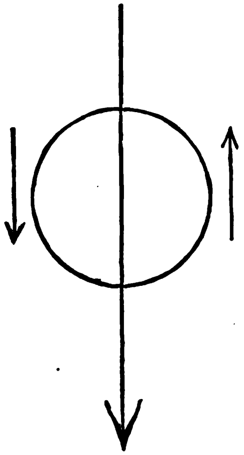
As I lift this book from the table, there is some power which resists the effort I make to raise it. That power has been termed by Sir Isaac Newton —and before his time, and by all who have succeeded him—vis-inertiæ, which Newton defined to be an innate power of resistance, by virtue of which the particles of which that book is composed resist the effort I make to take them from their state of rest upon the table.

According to Newton's own demonstrations, the earth's gravitation holds that book down upon the table, and therefore the earth's gravitation is an intrinsic part of that power of vis-inertiæ. And, according to those same demonstrations, every particle of the universe attracts every other particle : there is a connecting link between each one of them, and just exactly as the earth's gravitation attracts the particles of this book, so also every particle of the universe attracts these particles, and therefore, the earth's gravitation is not the only power attracting the particles of this book. Every particle of the universe is bound by a connecting link with it, and when I move that book, I, in fact, separate two distinct parts of matter which are by the power of gravitation held together—I exert a force to raise that book: with exactly the same force with which I raise that book, I, at the same moment, press with my foot downwards, and it is only because there are fewer connecting links between this book and the other powers of gravitation, that it is this book which absolutely moves, and the movement of the earth, as I press it in the opposite direction, is practically imperceptible. (Applause.)

I wish now to show you the action of a force between which and this power of vis-inertiæ there is a very slight difference. For this purpose I hoped to have had here on the table a glass of water ; but as that glass of water is not here, I beg you to imagine it here, and to believe the phenomena which I tell you exist, do exist in it—perhaps the water will be here before I absolutely require it. As I am now pouring out this water—at this moment I am afraid it is not sufficiently settled—but if anyone cares to come forward to see the character of the motions, they can do so. The great majority of those present, I dare say, are perfectly acquainted with it. As I hold up this glass, I have fixed on the rim a small piece of paper, and on the surface of the water, and quite close to it, I have placed another piece of paper.

The piece on the glass is now facing you, and the piece floating is close to it on the surface of the water, and as I give the glass a turn in this direction, the particle of paper on the surface of the glass is moved round with it, but the piece on the surface of the water remained, more or less, where it was before, because the particles of water in the glass endeavour to remain in the position in which they were. They were in fact held in the positions in which they already were, and resisted the effort made to move them in this direction. Now, the power which held the water is not exactly vis-inertiæ. The difference, when dealing with the phenomena of this glass of water, is exceedingly slight; but when we come to deal with the subject of the ocean, the difference is exceedingly important. We can all imagine this glass gradually to be increased in size, and more and more water put into it. After having been increased to a certain extent, then there would be a curve on the surface of the water, corresponding exactly with the curve of the earth's surface ; and we can imagine that glass increased in size, more water put into it, its shape changed, and, by gradual gradations, you might at length have a glass, or something to contain water, with all the water of the ocean in it, and of the same colossal configuration as the earth containing the ocean lying upon its surface. I cannot imagine any point at which the increase of size must necessarily be supposed to prevent the same forces which we see acting in this glass from acting upon the ocean, and the whole theory upon which these charts have been drawn is based upon the tendency of the water to retain its position. When we treat of this quantity of water as it lies upon the surface of the earth, we have in fact the earth's gravitation endeavouring to carry the water along with it, whilst the combined action of all the other powers of gravitation are tending to retain it in the position from which the earth's gravitation tends to carry it. To the extraneous power of gravitation I have applied the term Astral Gravitation, so that, on the one hand, we have terrestrial gravitation tending to carry the ocean along with the motion of the earth, and, on the other hand, we have a force of astral gravitation drawing back in the opposite direction. As regards the rotation of the earth, the force of terrestrial gravitation acts as a revolving force. As the earth rotates, it endeavours to revolve the water with it, whereas the extraneous force of astral gravitation, endeavouring at each moment to hold each particle in the position which it last occupied, acts as a retarding force, drawing the water upwards and backwards. Now, if it be once admitted that this revolving force does exist—of which I do not now pretend to offer a proof—but let it, however, be once admitted that it does exist—then it is simply a mathematical certainty that that force is at different distances from the earth inversely as the cubes of those distances; that is to say, as you recede from the earth's surface, that force not only constantly decreases, but decreases in a far greater ratio. If there be such a force, then we ought to expect to find that any objects which are round about the earth should lag more than the ocean does. We have one such body—the moon—at a great distance from the earth's surface, and in fact the motion of the moon is such that it does lag. As the earth rotates in the twenty-four hours, with that rotation endeavouring to carry the moon round with it, the moon lags constantly behind the positions to which the earth tends to carry it, and it takes twenty-eight days instead of one to revolve round the earth. Now if there be such a revolving force about the earth, it is only reasonable to suppose that there is the same revolving force about the sun; at least, we cannot imagine the moon to be carried round by such a force, unless the planets are also carried by a similar force round the sun; and as we have a lot of planets at different distances round the sun, we cannot help looking to see whether or not the actual velocities with which the planets revolve, do or do not corroborate the existence of such a revolving force. It is a mere matter of fact, and not of theory, that if you have about the sun such a revolving force as I have just described, and if you measure out the distances at which each planet is placed from the sun, at those distances take the square roots of the force which you have acknowledged to exist, then those square roots absolutely lay before you the mean velocities with which each planet actually revolves, or describes its orbit round the sun. (Cheers and applause.) These square roots give you the mean velocities of revolution for each planet. As I have looked into very many other more minute details connected with it, the attention I have given to it makes me consider that one fact quite enough to demonstrate the existence of that revolving force. I do not expect anyone else to do so, until they have looked more into the subject—until they have looked about to consider whether they can find any phenomena that might contradict it; and until anyone does that, I do not expect him to accept it as more than a coincidence; but anyone who does look into it, will find it not merely a coincidence. But to my mind, there is one thing which enables this revolving force and the force of astral gravitation to set at defiance all assaults upon it, on whatever grounds they may be based. It is this: that having admitted the existence of the force, it is a mere matter of fact, because, as far as theory is concerned, I really do not know how to go to work to reason upon the subject; but I find, as a mere matter of fact, that as the planets revolve, the force of astral gravitation retarding their motions is directly as the squares of their velocities of revolution; that is to say, that as the sun endeavours to carry the planets round with it, astral gravitation is retarding this motion, and the velocities with which they revolve, squared, give the forces of astral gravitation which oppose the motion in each orbit. It does so for this reason: that, according to this theory, the sun rotates, endeavouring to carry the planets with it, and that rotation carries the planets along their orbits, and as they move in their orbits, they are held in equilibrium between the centrifugal force of astral gravitation, and the centripetal force of solar gravitation; otherwise the planets would approach to or recede from the sun; and, therefore, supposing that the theory be true, then the force of astral gravitation is as the square of the velocity of revolution. Now, what I consider the clinching point of this, is the manner in which, when this is applied to the most ordinary phenomena about us, it explains phenomena which

I think never have been satisfactorily explained. I allude to one which I am not explaining for the first time. We are all accustomed to see a top spinning, and we take it as a matter of course that because the top is spinning it does not fall—we take it as a matter of course without caring to know the reason. When the gyroscope was first known, it was immediately asserted, and is still believed by many, that the motions of that gyroscope show that the laws of gravitation are imperfect, and that when the gyroscope is revolving at such a rate, the laws of gravitation are suspended, because it appeared that the earth's gravitation ceased to act upon it. As regards that gyroscope, I am not going to discuss all the phenomena connected with it, but merely allude to that power by which it is sustained whilst it revolves, as being the same force which keeps the top from falling, and that is the force of astral gravitation. I have not a top here, but let us suppose that this plate is a section of that top with the peg turned in this direction, and that it rotates in this direction. Suppose the velocity of rotation be, say, 8 in this direction; if the top were not spinning at all, the earth's gravitation would draw it down, but



when it begins to spin, if the resisting force of astral gravitation is equal to the square of velocity, then the resistance of astral gravitation is as the square of 8 on each side; but immediately the top moves from that position and begins to fall downwards—let us say with the velocity of 4, in a downward direction—then all the particles on that side of the top immediately, with this motion added to them, have their velocity increased; and that velocity of 4 added to the velocity of 8 makes the velocity of 12, whereas the velocity on this side is reduced to 4; so that the resistance of astral gravitation on this side is the square of 4, or say 16, whereas the force on that side is 144, say the square of 12; so that the force of astral gravitation on that side is 144 drawing upwards to 16 on this side drawing downwards. Thus there is a difference of 128 representing an excess in the force of astral gravitation drawing upwards, and unless the earth's power of gravitation, drawing it downwards, is equal to that difference of 128, the top cannot fall; it is then supported, and it is only when the top's rotation becomes so slow that the earth's power of gravitation dragging it down is greater than the difference between those two forces of astral gravitation, that the top can fall. (Hear, hear.) The top, when spun, comes to the ground on its peg, because when the plane of rotation is horizontal, astral gravitation draws equally all round in that plane; but when the plane of rotation is vertical, as in the illustration, its downward motion is resisted. The top, when spun, falls upon its peg because astral gravitation will not allow it to fall upon its side. When I look into this I think it must convince any one of the accuracy of the theory; but that is not what I so much depend upon. My chief dependence is on the evidence of facts discovered since the theory was published. Now we have gone so far, I think I may point out that, excepting additions I have made extending the application of accepted laws, I have been thoroughly orthodox, and have not gone away from anything that Newton or others have done with him. I have kept completely with them; but what I have done is simply this: I have applied exactly the same laws which Newton applied to the planets—precisely those same laws I have applied to every particle of water in the ocean. If Newton is not right, then I am altogether on a wrong basis, because the only basis which I have to go upon is the correctness of his theory of gravitation ; but if we once admit the existence of this revolving force about the sun, and that the rotation of the sun actually does tend, and not only tend but does carry the planets along their orbits, then we no longer require the other half of his definition of vis-inertiæ. I have told you that Newton says that the power which tends to keep this book at rest on the table is vis-inertiæ, but he also says that if I put this book in motion and throw it away, its vis-inertiæ has then a tendency to keep that book moving for ever onwards, with the motion which I have given to it, and that the planets have, for some unknown reason, and from some unknown power, received each a primary impulse, and that the vis-inertiæ of each planet tends to keep each one constantly moving in a straight line, from which straight line the sun's power of gravitation deflects them and keeps them in their orbits. But if once we admit the existence of this revolving force of gravitation, and that opposing forces of gravitation are keeping each one in its place as it goes along its path, then we no longer require the other half of that definition of vis-inertiæ. The latter half of that definition of vis-inertiæ is, I assert, without the slightest fear of anyone in this room rising to contradict what I say, a mere fiction. It is a fiction, and always has been a fiction. It is a fiction invented to explain phenomena for which no other explanation had been found; no practical evidence of its existence can be given in any physical phenomena. In the absence of the existence of this revolving force, that definition of vis-inertiæ completely supplies the vacuum. It was requisite to explain why the planets were not drawn to the sun by the sun's centripetal force, and that definition most completely fulfils the purpose; but if reason and argument show that this revolving force is in existence about the earth and about the sun as I say it is, acting just as we see its action in this glass of water, then we no longer require that fiction. Its imaginary action serves as a centrifugal force in theory, exactly the same as the retarding forces of astral gravitation, excepting this: that, for instance, if I throw anything onwards—this book, or a ball—in any direction onwards, that ball is held in contact with every power of gravitation there is in the universe. As I throw it and impart to it a certain amount of motion, that motion is imparted by the ball to the particles of the air and ground which it strikes, until the amount of motive force exerted is spent, and then it is stopped in its career; but even if the ball is thrown into free space, still there are those connecting links of gravitation which form that power termed vis-inertiæ, which endeavours to keep it at rest, and as it moves it is severing the connecting links with all other particles which are trying to keep it in its position, and therefore just as certainly its motion would be retarded if moving in free space as if it were moving along the ground. I have frequently had the remark made to me, 'Oh, but the stars are so far off, they are at such an enormous distance from the earth, that their power must be quite imperceptible; they can scarcely be felt here.' We know what the power of the earth is, and the gravitation of the sun, of the moon, and of the little planets. The stars are far off, but we don't know how large the stars are. We do not know the number of these stars; but all these, even if infinitely small forces, make up a very great power; and, to illustrate how very erroneous our first impressions on this subject may be, I wish to offer an illustration on a similar point on which all our ideas have, during the last twenty-five years, been completely revolutionised. Twenty-five years ago it was the opinion of every one, no matter how eminent in science or otherwise, that at the bottom of the ocean there could be no life, there could not exist any living animal or fish of any description, because it was known that the pressure at the bottom of the ocean was enormous, and it was supposed that no life could exist under such pressure. About the year 1854*—I think in that year—a small vessel of war called the *Bulldog* went out on a cruise, in command of Captain McClintock, afterwards Sir Leopold McClintock (applause), and in that vessel a scientific gentleman who had long been quietly making his observations among the mountains of India—Dr. Wallich (applause)—obtained permission from her Majesty's Government to go as a volunteer on board that vessel, in company with Captain McClintock, for the purpose of dredging and making scientific observations, and attempting to ascertain whether or not the views which he had formed could be corroborated. As a result of that voyage, Dr., now Surgeon-major, Wallich published a small and unpretending pamphlet, in which he gives his reasons, which he says are absolutely conclusive of the fact that the bottom of the ocean teems with life, and not only teems with life, but there are found the most delicate organisms, more beautiful and delicate than you find generally on the surface of the earth; and as a reason for the possibility of the existence of that life under the enormous pressure of four or five tons weight on every square inch, he suggests that the animals in fact breathe that water: it permeates their systems so that the pressure is inside them as well as outside them (applause), and that they are therefore unconscious of it. We were astounded how any living creatures could exist there, but they did exist there, and were unconscious of any difficulty. Dr. Wallich pointed out that the only thing which would render it impossible for that life to exist there would be the necessity for these living organisms to have within them some parts into which the water could not enter, but otherwise there was no reason whatever why the most delicate organisms might not be found living under that enormous pressure as people do here, under a very trifling pressure. (Applause.) Ladies and Gentlemen,—What was then said by Dr. Wallich was for years treated with contempt. (Voices: 'Shame;' 'No, no.') Well, I will not say with contempt—but a great many people knew too much, and had thought too much on the subject, to be ready to understand the bearing of it; they had made up their minds that life could not exist under that pressure, and they thought that there must be some mistake, and no attention was given to Dr. Wallich's investigations; but the investigations made by the *Challenger* have triumphantly proved every word that Dr. Wallich wrote more than twenty years ago.* (Loud applause.) And now, I have simply made a slight allusion to this oceanic pressure for the purpose of illustrating the power of astral gravitation, so as to incline you to admit the existence of an enormous power about us of which we are unconscious. It has been constantly said that the stars are at such a distance from the earth, that their power must be only slight; but if we once suppose that the universe is infinite—I do not pretend to say that it is—if we once admit that, then all these very small particles of power make up an infinite power, and if you compare the force of this infinite power with the mere force of the earth's gravitation, it is as if you were to compare the hour, or hour and a half, during which we are here this evening, with eternity. That power is so vast, that when we have to deal with such a power, we have to recognise that any movement may be controlled by slight differences in its action—if we make the slightest movement there is a complex changing of our connecting links with particles of that infinite power—and thus we see that the power of gravitation is capable of the most stupendous results, even on a very slight movement. Since April last, results of investigations have been brought before me—investigations, with which I ought to have been acquainted long ago—investigations for some years past carried on and conducted at St. Thomas's Hospital. The gentleman who originated them, and who has with untiring attention carried them on, is Professor Rainey, with whom I am scarcely acquainted—I have only met him once—but the course of his investigations is this: He places small quantities of water in glass vessels of different shapes, and in every conceivable manner brings the microscope to bear upon them. He has kept for years the same quantity of water, hermetically sealed, in the same small glass; he has also changed the nature of the contents, and put glycerine and other matter in the water, and all with the same result; no matter what attempts he has made to prevent the action of heat, or any difference of temperature acting on the water, however he attempts to keep the water still and prevent forces coming to bear upon it, the microscope reveals the existence of currents and countercurrents in every case, so much so, that, when he has arranged the microscope at a certain focus, he will find a wave going on in a certain direction (applause), and he will then quietly say to you, if that motion is going on only in that direction, all the water on this side would be carried to that side, which is impossible—there must be countercurrents to return the water which you see flowing in that direction. He will then change the focus, so that you look to a greater depth. The quantity of water is very slight, and as you go on changing the focus, you begin to see a clearly defined motion in the opposite direction—you will come upon a returning current. And after years of careful investigation, Professor Rainey has been unable to get the water to keep quiet—it will circulate, no matter what he does, and Professor Rainey has attempted an explanation of it. I am not going to say that that explanation is correct—I will leave that for others, who have given more attention to the subject—but this I will say: I think, in the last paper published by Professor Rainey brought to my attention, he says, that the only way in which he can account for this is, that particles of water have a power of cohesion among themselves, and that there is an external force of gravitation trying to derange it, and not acting as that power of cohesion acts, so that the particles are constantly changing their positions. Now, these forces of cohesion and gravitation, as Professor Rainey describes them, are identical with those of terrestrial and astral gravitation; for terrestrial gravitation really is the cohesion of the particles of the earth together, and astral gravitation is that, to which Professor Rainey gives the term gravitation, acting upon it, and deranging its action, causing a conflicting action which he is not able to stop by changing the temperature, however he attempts to do it. (Hear, hear.)

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* 1860. *See* page 34.

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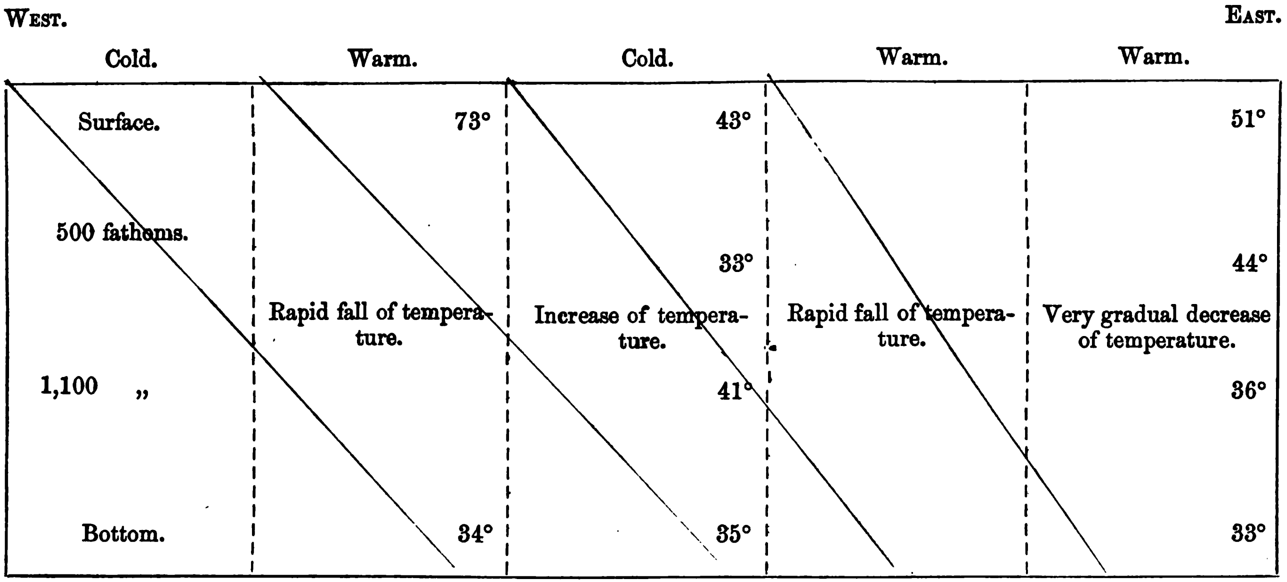
* 17 years. *See* page 34.

I do not wish at all to go beyond matter of fact in anything I say to-night; but in alluding to this subject, I think I may be allowed to make a few remarks, which are these: that I have no doubt whatever that Professor Rainey has most clearly realised to himself the marvellous power of these conflicting actions of gravitation, and that he has made them the subject of the greatest study in hours which are, in fact, stolen from his earnest labours in the dissecting room. (Hear, hear.) In these hours he has, with great devotion, studied the subject, and I must venture to anticipate that at some future time these investigations which he has made will be looked back upon as what I think he must really believe them to be, although I have not spoken to him upon the subject. (Hear, hear.) It is not many years since the then startling fact was discovered that the blood is in circulation in our veins, and I have no doubt that in the future, and perhaps in no very distant future, those investigations which Professor Rainey has carried on, will be found to form the first step towards showing the reason why the blood does circulate, and what the forces are that cause the circulation; and it will then be perceived that the surgeon and the astronomer have, in fact, been investigating the action of the self-same force—that infinite power of gravitation which moves the planets, and is in action throughout the universe. (Loud applause.)

I find the time runs so very rapidly, that I shall have to be very brief, and therefore I must simply take a few points in connection with the circulation of the ocean to prove this conflicting action of astral and terrestrial gravitation. As a matter of theory, the force of astral gravitation draws the ocean upwards and westwards throughout the equatorial regions; whilst at the same time the power of terrestrial gravitation draws it downwards and eastwards along each temperate zone, so that the water about each temperate zone sinks down. It thus, in the under strata, flows to the equator to rise upwards, and to the Polar regions, there also to rise upwards, and returns along the surface of the ocean from the equator and the pole to the temperate zone. This is a matter of theory, which any one who examines into the action of these conflicting forces will admit, I think, to be in theory correctly calculated (hear, hear); and it is under that action that this chart and sections are drawn. Here in this section (Plate III.) it is described along the equator as water running westwards and rising upwards; and in the temperate zones as water running eastwards and sinking downwards. These (Plate II.) are vertical sections, and the horizontal section is supposed to be taken across them half way in the depth of the ocean ; but this is a matter to be decided by practical investigation, and not by theory. The axis in each district is in fact broken by islands and variations of depth, but the water about each fragment of the axis has a circulation exactly similar to that of the whole district; and to make this very short, as time goes so rapidly, I will take only one point, which I select from more than twenty, each one I think just as valid; but if one is made clear and distinct, it may do as well as twenty, especially as our time is short, and those who care can go further into the subject. (Hear, hear.) I will therefore take the sinking of the water in the temperate zones, and not endeavour to apply it to all the ocean. When these sections were first published, it was considered by everybody that no such circulation could exist, that there was no reason for the existence of it, and that it was perfectly well known that there were circumstances which made it utterly impossible for such a circulation to exist. I should have been completely overthrown by the fact of the theory necessitating this circulation, but for the evidence of other phenomena which showed me that the theory must be true, and I published these distinctly, to leave the matter to the test of subsequent investigations. (Hear, hear.) Some years after the publication of these sections, it was found that there is a great mass of warm water in this part of the North Atlantic (the eastern part of the temperate zone); all through this part it was found that at the depth of upwards of 4,000 feet the water was warmer than at less than 2,000 feet, near the Equator, and therefore it was evident that cold water was rising at the Equator, and the water, warmed on the surface, was sinking in that part of the temperate zone. What was previously supposed to be the circulation of the ocean was said by some to be caused by the winds, and others attributed that same supposed circulation to difference of temperature. Those who support the wind theory now say it is natural that there should be this accummulation of warm water, because the westerly winds are driving warm water against the coast. And those who support the temperature theory now also say it is natural that the water should be found here, for the water gets warmed upon the surface, and as it streams away from the Equator, and reaches this part of the ocean, the ocean being narrower in that part, the warm water moving northwards is compressed, and is therefore found at a greater depth. (Hear, hear.) I am not now going to argue against either of these points ; they are fair and reasonable, though I think that even as a mere matter of argument, the question might be carried in the other direction—in fact, I think it has been carried in that direction ; but, nevertheless, both these appear reasonable and plausible arguments. (Hear, hear.) A very eminent member of the scientific world, who supports the temperature theory, when he ascertained that the narrowing of the ocean causes, as he supposes, that depression, made an observation which I think is of such a nature as to be worthy of the greatest praise. That gentleman is exceedingly well known throughout the world—I allude to Dr. Carpenter. (Loud applause.) He made manifest the sincerity with which he entertained his views, by stating that as it is the narrowing of the ocean which causes the accumulation of warm water in that region, it was quite obvious, that as the warm water streams away from the equatorial regions of the Indian Ocean and gets south of the Cape of Good Hope and Australia, it was obvious that as the ocean widened out there it would be found that the warm water, instead of sinking down to a greater depth than at the Equator, would be found to spread itself out over the surface of the ocean as it streamed down to the Antarctic regions to be chilled. (Hear, hear.) That, I think, is the fair way of stating the theory; and whenever a theory is sincerely believed in, I think it is not only allowable, but proper, to extend it as Dr. Carpenter has done in this instance, and then look for the proof that may be brought against it. (Hear, hear.) Within two years after the extremely interesting Paper, in which the foregoing view was expressed, was read before the Royal Geographical Society by Dr. Carpenter—a Paper full of matter of interest and importance, even supposing this to have been a mistake—within two years the *Challenger* passed through those regions, and made observations which had not previously been made, and, as a result, it was found that about here (to the South of Australia), where there is no coast line—between Patagonia in this direction and Patagonia in that direction—there warm water was found 4,000 feet below the surface, warmer than water within 2,000 feet of the surface at the Equator in the Indian Ocean; in fact, precisely the same phenomenon as found in the North Atlantic, making it evident that it is not the narrowing of the ocean that causes the sinking of the water. (Applause.) My impression is, that when that remark was made by the eminent philosopher whom I have mentioned—my impression is that he had in view this theory of astral gravitation, and that he selected that part of the ocean, supposing that this theory would there be completely annihilated. (Hear.) A more fair point to select there does not exist in any part of the ocean, simply because there are no coast lines to interfere with the action of these conflicting forces, and if they are not found to accord with the theory in that part of the ocean, they can no longer be maintained in any other parts. (Hear, hear.) In some parts any difference might be cleverly explained away; but there are no coast lines in that part of the ocean, and therefore the theory ought to stand completely in accordance with these sections. Now, not only was warm water found sinking down in that part of the ocean to the south of Australia, but we have evidence of eastward motion whilst the water sinks. The stream of warm water coming down between the Island of Madagascar and the coast of Africa, makes its way to the south of the Cape of Good Hope. Here there is a great mass of warm water coming down the central Indian Ocean. Between these two masses of warm water there is brought from the Antarctic regions a mass of cold water. (Hear, hear.) Time forbids me to enter into the reason why these masses of water, of different temperatures, are there, or to trace them to this position; I only have to deal with the fact that they are in that temperate zone, which is the zone in which the water should be found, according to that section. There are theoretical reasons for the existence of this stratum of cold water in every ocean, but here its volume is perhaps increased by the fact that, as these masses of warm water pass away from the Equator, the Island of Madagascar forms, as it were, a lee, which enables a greater mass of cold water to get between them than would otherwise be there.

Now, I ask you to watch this, as I have no special diagram. You have simply to consider that all along the temperate zone the water moves eastwards and sinks downwards, and here you have a free extent of ocean. There may be currents moving in different directions in it, but the whole mass has a motion eastwards and downwards, and if that be so, if you have water of a cold or a hot temperature on the surface, then that water must be found at a lower depth again; so that if you have cold water west of the hot water, then when you sink your thermometer, you find cold water under the hot water; but as you have very warm water here, with cold water here east



Vertical section of the Temperate Zones, showing the inclination of the strata of water eastwards from the surface to the bottom of the ocean.

of it, then as the warm water is also going eastwards and downwards, it ought to be found under the cold surface water, unless the temperature disappears so rapidly as to render that impossible. (Hear, hear.) Now, in point of fact, here the *Challenger* found a temperature on the surface of 43°, showing that the water here came from the Antarctic regions. As that water moves along from the cold regions of the south the surface gets warmer, as it enters a warmer climate, but below the surface the heat has not penetrated, and in fact at a depth of 500 fathoms, a temperature of 33·7° is found; but on going on and getting down deeper, it was found that the water got warmer, and at a depth of 1,100 fathoms, a temperature was found of upwards of 41°; that is to say, you find a volume of water here of 33°, and below it 41°, that is this warm water slanting down eastwards in that direction. Under this other mass of warm water which lies east of the cold there is no doubt that colder water does exist, but how much colder I am not prepared to say; but I have no doubt it is the same stratum which divides the warm water on the surface. The warm water was found sinking downwards in this part (South Australia); so that in fact the temperature here makes itself felt by an enormous mass, which even at a depth of 4,000 feet is found to have a temperature of 40°, whereas here this water is found to have a temperature of only 33° at 3,000 feet. Through that region you find the water sinking not only downwards, but eastwards, exactly as is represented by this section (applause); and I do not think it possible to have anything more distinct, taking the ocean at any part, to enable you to arrive at the conclusion that if you find warm water lying to the west of a mass of cold water, if there be a sufficient difference of the temperature to prevent that temperature being carried away as the water sinks, then below the cold water the thermometer will show the existence of the warm water; and if you have warm water on the surface, and cold water to the west of it, you will find a surprising difference of temperature. It is rapidly diminished as the depth increases, because you get to the stratum of colder water; if, on the other hand, you have a great extent of warm water, as in the middle of the Indian Ocean, and go eastwards in it, so that lying west of your position you have an extent of warm water, when you sink your thermometer you will find a very gradual difference of temperature, and you go to the very bottom of the ocean without finding the temperature so cold as you find it at a moderate depth where cold water lies on the surface not far west of your position. (Applause.) I have gone through all these Reports, and taken them exactly as they were published from time to time, and as they were sent home by Captain Nares, and they recorded the facts as to the circulation throughout this special part of the ocean, which is the battle-ground selected by that very eminent man, Dr. Carpenter, and a fairer battle-ground for his theories you could not possibly have selected. (Applause.)

Ladies and Gentlemen,—I have before me headings which would carry me on for far more time than I can expect your attention; and not only so, but I hope there are some here present who will give you the benefit of hearing what they have to say on these subjects, and therefore I will not go on any further; but as this programme I have given out refers to the winds and the tides, on which I have said nothing, and only taken a point as to the circulation of the ocean, which is only one point out of twenty, I ask anyone who gets up to reply to keep to the matter to which I have referred (hear, hear); though, if there should be any one who should afterwards wish to consider the matter further, I shall be glad to do so. I will now ask if any gentleman wishes to speak on the subject. Perhaps, strictly speaking, we ought to have had a Chairman; but I have no doubt that the only laws we require are simply those of good feeling and common sense, and I feel confident that whoever speaks will remember to reply to that part of the lecture I have given, even if they are disappointed that I have not spoken upon points of their own referring to the winds and tides. I should have been glad to do so if time permitted. (Loud applause.)

Dr. Wallich:

Ladies and Gentlemen,—I can only assure you that when I came here this evening I came without the slightest idea that I should be called upon to address you, and there are two reasons why I should not. I don't pretend to be a sufficient critic, and therefore I think it is arrogant and presumptuous to offer any decided opinion on what we have heard this evening from our friend Mr. Jordan. (Hear, hear.) I had the means of knowing for many years and studying the subject on which he has been engaged, and I can only say, that so far as my judgment goes, it does not appear to me that it is at all necessary to demand any special information to understand what his theory is, and not only to understand but actually to admit that there must be a great deal of truth in it. (Hear, hear.) I, for one, can certainly not see how anybody who admits the fact of the closest connection between every particle of matter in the universe and every other particle—I do not see it possible to deny his theory; whether it explains the phenomena which he attempted to explain, I dare not say. He has conscientiously done his best to bring his theory forward, and where a man makes a single-handed effort in the midst of numerous difficulties—when he is not a paid servant of the State, but merely a man working for the love of science—I think it somewhat hard that his labours should have been ignored as they have been. (Loud applause.) I think it is high time that they who profess to pooh-pooh him should be called upon to argue the matter out, and let us know what their reasons are for so clearly denying the truths he has brought forward. (Loud applause.) Gentlemen,—These are not the times when a man can get up and stand on a pinnacle on which he has not raised himself by his abilities. (Loud applause.) I say these are not the days that men should turn round and say, 'Bless me, what does it mean? We know all about it.' I defy anybody to deny what I have said—that there is far too much tendency in our scientific body to keep out of the magic circle those who do not belong to it, and a desire to throw cold water on researches honestly carried out. (Loud applause.) My friend Mr. Jordan has been speaking of me in connection with this subject. He told you that some seventeen or eighteen years ago—in 1860—I went out to determine whether or not certain ideas which I had formed during several voyages which I had made, and which led me to believe that those ideas were true, were or were not true. Up to 1860 it was absolutely denied that anything in the shape of animal life could exist at the bottom of the sea; and not merely denied, but reasons were given to support that denial. All that I did, without going into the abstruse question of physics, was simply to show that animal life to be there, and that conditions might be made to accord with animal life; that there was nothing in the condition of things and feeling at the bottom of the ocean which would prevent the existence of animal life. (Applause.) The credit of that has been taken by others. I think that as this statement has been made in the one case, it shows there is a probability—to compare large things with small—that the same kind of treatment has been extended to Mr. Jordan. (Applause.) I hope that the time has come that he may receive that recognition of his valuable services which I, for one, think he most richly merits. (Loud applause.)

Mr. Jordan:

Just one word to supplement what Dr. Wallich has said. I thank him exceedingly for the very kind words he has spoken of me. I must admit this one point, that it is rather premature to find fault with those views not being generally received; it is only natural that when there is such a distinct and important difference between a fundamental point always believed in, that people must be slow to receive it (hear, hear); and it is those who know most about what is entitled to be termed orthodox philosophy, and have been brought up in it, that distinctly see the bearing of what I have published, even if I had left the point of variance untouched; and that is the manner in which it clashes with the old theory of vis-inertiæ—and that I have tried to point out to-night; and therefore it is only natural to expect that there should be a good deal of opposition (hear, hear). I hope some one else will offer some further observations, either for or in opposition to my views. (Hear).

Dr. Rae,—

I merely came as a listener to Mr. Jordan's lecture, and had not thought of saying a word on the subject, nor have I even read the whole of the 'Challenger' reports. The subject seems one of so much importance that I could not think of discussing it without some further, some deeper, consideration. My observations, as far as they go, have been confined to narrow seas, where the currents are much influenced by the winds, and by the water being more salt in one part than in another; therefore the results of observations made in narrow seas may in no way resemble what takes place in wide open oceans, of which Mr. Jordan speaks, and of which, as I have already said, I have little or no personal knowledge.

We must all thank the lecturer for the new ideas he has brought before us, but they cannot well be discussed satisfactorily on the spur of the moment.

A gentleman, whose name did not transpire, said that the last speaker had thrown a new light on the subject, namely, the difference that saline matter makes in the water.

Dr. Rae:

That is a strong point, and I only speak of what I have seen.

I do not profess to be scientific, but it is well known that, although Arctic water is much colder than tropical water, it is also much less saline, especially that which is on or near the surface ; the consequence is, that when these meet, the latter, although the warmer, is, owing to its greater saltness, the denser of the two, and therefore sinks below, not of necessity to the bottom.

I have crossed the Atlantic pretty often, and on one occasion, when we had kept somewhat to the south of the usual course so as to avoid icebergs, I saw these two waters meet—that from the Tropics (usually called the Gulf Stream) of a brownish colour, rushing in great eddies into the beautifully clear water from the north. I examined these eddies of currents with the thermometer; the temperatures varied, if I remember correctly, as much as 15° or 20° of Faht., perhaps more. I could not then test the question, but it is my idea that when these currents met, the warmer water descended below the colder.

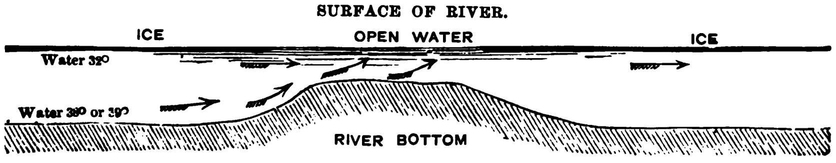
The last Swedish Expedition to the Arctic Seas has brought information that much warmer water than that at the surface was found at a very considerable depth, and then a colder stratum between that and the bottom—a fact previously noticed by Mr. Leigh Smith some years ago.

How does this warm water come to the surface again? or does it do so at all? My idea is this: If the current comes to a part of the sea less deep than elsewhere, the current, in flowing over this shallower part, must tend to bring the lower and warmer water towards the surface, and mingle it with the colder water previously there.

When Francis Joseph Land was discovered by the Austrian Arctic Expedition, Payer (one of the officers) found a large pool of open water early in April as far north as 82° lat., in which were great numbers of water fowl feeding and resting on the adjacent cliffs. As no mention is made of a strong current, I believe the cause of this open pool was, that the water was shallower there than elsewhere.

Strong currents will, we know, keep large spaces of water open in the coldest climates, and two cases may be mentioned—the one opposite Repulse Bay, to the extent of about 40 miles in length; the other (smaller) at Bellot Strait—yet in neither of these, although much further south than Francis Joseph Land, are any number of birds found in April, probably at no season, because the strong currents prevented the small animalcules on which the birds fed from coming to the surface, or other somewhat similar reason.

In nearly all the larger American rivers an open space of water is occasionally to be met with all the winter, for which one cannot at first account, because the current is not strong; on sounding, however, that part where there is open water is found to be much shallower than that immediately above, and the warmer water (say at 38° or 39°) is carried obliquely upwards so as to mingle with the colder water of 32° or 33° at the surface, and thus prevents freezing for a greater or less distance on the river.



There is an excellent illustration of this in the River St. Lawrence, a few miles above Lachine, in Canada ; a channel of considerable width, but very shallow, separating an island from the main shore, never freezes over in winter—although the current is very slow, and there is ice a foot or two thick within a short distance. If the elevation of the bottom of a river prevents the water while flowing over it from freezing, why should not a hill or mountain in the bottom of the sea produce a similar result?

Mr. Jordan:

I think in Dr. Rae's interesting observations there is nothing at variance with the theory I have put forward, but only a different action, which may co-exist with the action of the forces I have described. According to these, the water rises upwards in the Arctic regions as he has described. He has described the matter, and has spoken from more experience than I have had, inasmuch as he has been in those regions, and knows more of them probably than any man living. (Hear, hear.) I must apologise for saying that, because it is not my place to introduce Dr. Rae. He is well known, whereas I am unknown ; therefore, I must apologise for saying anything of the sort. (Hear, hear.)

Dr. HADEN :

I came here to-night in the full hope of hearinga strong discussion on the subject, but your opponents seem not to have put in an appearance ; and therefore I suggest that the meeting should be adjourned, until they think proper to appear. (Hear, hear.)

Mr. JORDAN :

There seems to be no intention of any further discussion.

Dr. HADEN :

I desire to move a vote of thanks to Mr. Jordan for the very able, logical, and interesting lecture we have heard to-night. (Applause.) I hope we may have the pleasure of hearing him again very shortly, and I trust that there may be a thorough discussion of the subject, as I think it is time that it should be so. (Hear, hear.) I thank the lecturer very much for the able manner in which he has dealt with the subject. (Applause.)

Mr. JORDAN :

Ladies and Gentlemen,—I thank you most sincerely for the very kind and courteous attention you have given to all I have had to say, and for the opinion Dr. Haden and others have expressed towards me. (Hear, hear.)

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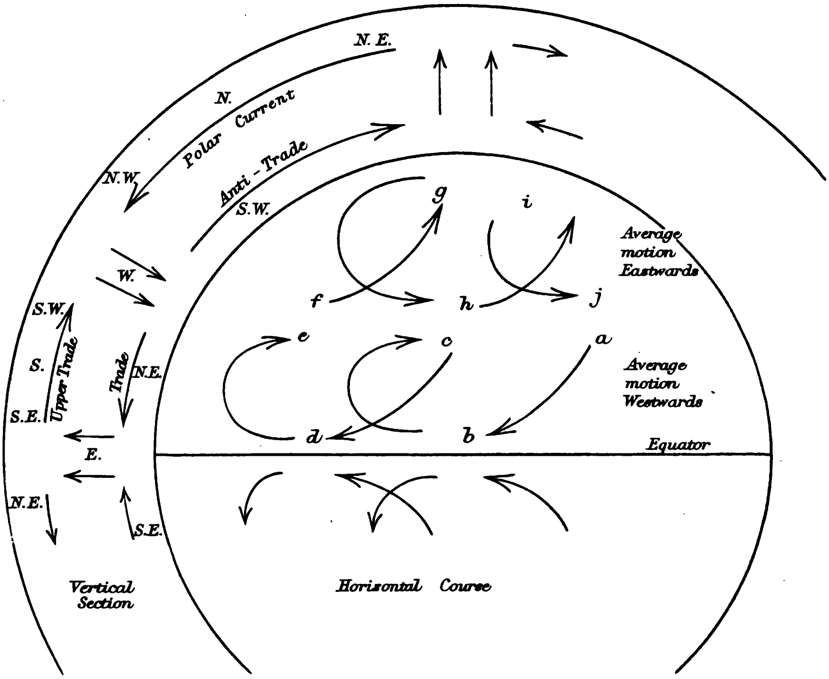
THE WINDS



Note.

The above represents also the normal circulation of the Ocean; and it is mere matter of fact that through the central parts of the Ocean, where its circulation is not deranged by the coast lines, the surface sets from the Equator and from the poles to the temperate zones against the average course of the winds.

A LECTURE

ON THE

WINDS, OCEAN CURRENTS, AND TIDES

AND

WHAT THEY TELL OF THE SYSTEM OF
THE WORLD

SECOND PART

DELIVERED AT WILLIS'S ROOMS, KING STREET, ST JAMES'S

ON THURSDAY EVENING, DEC. 20, 1877

BY

WILLIAM LEIGHTON JORDAN, F.R.G.S.

LONDON
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1878

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AND PARLIAMENT STREET

THE

FOLLOWING PAGES

Are Dedicated to the Memory of

JOHN KEPLER

LECTURE.

LADIES AND GENTLEMEN,—

WHEN Ptolemy and Tycho Brahe spoke of the sun rising and setting they really believed they were speaking the simple truth. But now, although we continue to use the same mode of expression, we know that it is not accurate. To them it was a mere matter of 'common sense' to believe the sun really to be in motion through the sky as it passes from east to west over our heads; whereas to us it is a mere matter of 'common sense' to know that the sun has not the motion they supposed it to have; but that, on the other hand, the earth is absolutely in motion, revolving around the sun and at the same time rotating on its own axis.

Just as we continue to speak of the sun rising and setting, we also continue to speak of sound travelling through the air; and not very long ago it appeared to everyone a matter of 'common sense' that as they listened to a speaker sound travelled from him to them. It is, however, now known that sound does not travel at all: that the vocal organs are endowed with the power of giving varied vibrations to the air, but not with the power of making any sound. Whereas the ear, which cannot impart vibrations to the air, is endowed with the power of making sound from the vibrations which it receives. And, as the ear keeps to itself all the sound it makes, there is, therefore, never any sound in the air outside our ears.

I am justified in asserting that we are not all imbued with this view of the subject as a mere matter of 'common sense,' for I recently heard, in the hall of one of our leading scientific societies, a clever and learned discussion as to how sound is reproduced in the far end of a telephone; and during the discussion the speakers at times evidently forgot that the question really at issue is as to how the vibrations of the voice, or other instrument, are conducted by the telephone; and that there never really is any sound in either end of the telephone to be reproduced at the other end.

The fact is that what is 'common sense' in one epoch may cease to be so in a more enlightened age; and also what is 'common sense' to one person may not perhaps, even at the same time, be so to one who has given more attention to the subject.

I have alluded to this fickleness of 'common sense' in the hope that it may induce you to allow the opinions you have brought with you to-night regarding the causes of the winds and ocean currents to sit somewhat loosely in your minds whilst I endeavour to bring fresh evidence to bear upon them.

About two hundred years ago Dr. Edmund Halley read before the Royal Society a paper in which he described the course of the winds within the tropics; and suggested that the Trade Winds might be due to the tendency of the heating action of the sun to draw the air towards its position in the under strata, to rise and overflow where the heating action is greatest, and stream away in the opposite direction in the upper strata. And the constant shifting of the sun from east to west round the earth, by which its heating action is constantly being moved westwards, is that which gives to the Trade Winds, according to Halley, a NE. and SE. instead of a N. and S. direction.

About fifty years after the reading of Halley's theory John Hadley brought forward a modification of it, making a less direct action of the sun's rays the cause of the Trade Winds.

He pointed out that the sun warms the surface of the earth all round the equatorial regions, leaving the polar regions cold; and that the air is heated by the warm surface on which it rests, and thus caused to flow from N. and S. to the equator; whilst the increasing velocity of rotation gives these winds, which would otherwise flow from the N. and S., a tendency to lag westwards, and thus to flow from NE. and SE.

According to Hadley's theory the earth's rotation would not affect the air unless it were set in motion by some other cause in such a direction as to make it approach or recede from the equator; and as there is just the same reason for the upper or return Trade Wind to blow from SW. in the northern hemisphere as for that below it to blow from NE., a motion westwards in the lower strata is supposed to be counterbalanced by an equal motion eastwards in the upper strata. But, according to these Sections, showing the course of circulation which the conflicting action of astral and terrestrial gravitation tends to cause, an average motion of the air westwards in the equatorial regions is counterbalanced by an equal motion eastwards in the temperate zones. The air is also drawn away from the poles in the upper strata, thus causing an indraught to the poles in the under strata; whilst at the same time the air is drawn upwards over the equatorial regions, and thus caused to converge towards the equator in the lower strata and to diverge from the equator in the upper strata.

If this conflicting action of gravitation were the sole cause of the circulation of the atmosphere the Anti-Trade Winds would then be found to be just as steady as the Trade Winds; but in fact the heating action of the sun's rays acts in concert with astral gravitation in the formation of the Trade Winds, whereas it is constantly tending to reverse the Anti-Trade Winds; and the extent to which the Anti-Trade or SW. wind predominates over the NE. wind in the temperate zone of this northern hemisphere shows the amount by which the power of the conflicting action of gravitation exceeds that of the heating action of the sun's rays; for under this latter action the NE. wind should blow across the temperate zone from the cold to the warm parts of the earth's surface.

The recent attempts to foretell the weather in the West of Europe by telegraphic warnings of that prevailing on the coasts of the United States is a practical abandonment of Hadley's theory, and a practical adoption or partial adoption of the theory of Vis-Inertiæ. For those warnings are based on the idea of the whole mass of the atmosphere in the temperate zone having a motion eastwards. And a further adoption of the new theory will have been made as soon as similar warnings are attempted in the equatorial regions; for there the warnings will have to be given, not from the coast of Brazil to the Gulf of Guinea, but from the latter to the former, for there the whole mass of the atmosphere moves westwards.

Ten years ago it was asserted by all authorities on the subject that the upper Trades blow away from the equator from SW. and NW.; that is, in exactly the opposite direction to that of the Trades converging below them. I, however, then fully pointed out that the reason for that assertion is to be found in the fact that Hadley's theory requires that they should have that course, and not in the evidence of facts.[1]

A few months ago I heard an interesting lecture delivered at a meeting of the Royal Geographical Society, in which Dr. Carpenter alluded to the phenomenon known as 'the rotation of the pendulum' as a proof of the correctness of Hadley's theory; because, as he pointed out, when set oscillating along a meridian it lags westwards as it swings towards the equator and advances eastwards as it swings from the equator; and thus the plane in which it oscillates gets the motion of rotation.

The phenomenon is this: suppose my shoulder to be the north pole, my outstretched arm a meridian extending to the latitude of London; and there I now set this pendulum swinging N. and S. (as it swings in the direction in which my arm is extended, or along the meridian). I now move my arm round to a position at right angles to that in which the pendulum was set swinging—a change which the true meridian takes six hours to accomplish. You now perceive that the pendulum is swinging at right angles to the meridian, or E. and W. But if I now set it swinging E. and W. it will change from that to N. and S. in exactly the same manner.

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[1] The argument is republished in *The Winds, and their Story of the World*, published in April last.

The conditions under which the phenomenon is thus illustrated are not exactly the same as those resulting from the earth's rotation, for in the latter case the phenomenon is complicated by the existence of the curve of the earth s surface from the pole to the equator. But, in consequence of that curve, the pendulum will, in fact, change its direction more easily when set swinging E. and W. than when set swinging N. and S. Whereas, to prove Hadley's theory true, it ought not to change its direction at all when set swinging along a parallel of latitude. The phenomenon rather tends to show that Hadley's theory is only one half of the true theory; it is a stepping-stone from Halley's theory to that of Vis-Inertiæ. The pendulum, in fact, knows nothing of the varying velocities of motion below it. The only onward motion which affects it is that of the point from which it is suspended; and, as it is equally free to swing from that point in any vertical plane, it keeps to the plane in which it may be set swinging, in the absence of any reason for changing it; and thus the direction of its oscillation as regards the meridian is constantly changing. For the tendency to retain the original plane of oscillation causes the rotation on the vertical axis passing through the point of suspension.

Now, as the pendulum tends to keep to the same plane of oscillation in whatever direction it may be carried onwards by the motion of the point from which it is suspended, that seems to make it evident that, as its onward motion is communicated to it solely by the point of suspension, it must be constantly tending to retain its position as well as its plane of oscillation.

You must not too readily admit that this is not only a 'common sense' but also a true view of the matter; for when once you do so you will then have virtually admitted the whole of the new theory of Vis-Inertiæ, for these Sections are drawn on the supposition that each particle of the air and of the ocean has a tendency to retain the position from which the earth's rotation tends to carry it, just as I say that the pendulum tends to retain its position as well as its plane of oscillation. The plane of oscillation is partially preserved because the earth's gravitation allows the pendulum to swing with equal freedom in any vertical plane; but the position is not retained, because it must change with that of the point of suspension.[1] But the particles of air and water, not thus rigidly held to the earth, are comparatively free to move and circulate within the bounds in which they are held by the earth's gravitation.

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[1] If it could be proved that the pendulum, if severed from its point of suspension, does not lag as it falls to the ground, then the new theory of Vis-Inertiæ would be proved to be false. But in fact, when severed from the point of suspension, the pendulum does immediately begin to lag, just as a stone dropped from any height lags as it falls to the ground.

I will now pass on to the question of the circulation of the ocean.[1]

These diagrams, showing theoretical Sections of oceanic circulation,[2] are drawn in accordance with precisely the same conflicting action of gravitation as that diagram of theoretical Sections of the winds;[3] but in the latter no allowance is made for the derangements caused by the inequalities of the earth's surface, whereas in the oceanic Sections allowance is made for the manner in which the circulation is broken up by the interference of the land.

About the central parts of each ocean, however, this diagram of the winds is, theoretically, just as applicable to the ocean as to the atmosphere. And practically it ought, indeed, if the theory be true, to be found in some respects to be more in accordance with existing oceanic currents than with their counterparts in the atmosphere. Because, for instance, though the sun's rays are constantly tending to derange the Anti-Trade Winds, they obviously cannot in the same manner be tending to derange the analogous currents at the bottom of the ocean. And therefore, though the Trade Winds are more constant than the Anti-Trades, the analogous currents diverging along the bottom of the ocean from the temperate zones to the equator and the poles ought to be equally constant.

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[1] Some confusion has been introduced into the discussion of the causes of oceanic circulation by the fact that almost immediately after I brought forward a theory of circulation which is purely a 'gravitation theory,' showing how a circulation may be caused by the direct and sole action of gravitation in a manner never before imagined by anyone—a theory really new, and really a gravitation theory—just then a theory was announced under the name of the 'gravitation theory,' and received by the Royal Geographical Society as a new discovery. But in fact this latter 'gravitation theory' is nothing but Maury's 'specific gravity' or 'temperature theory,' which, under a new name, remains nevertheless virtually the same as before. Hadley's theory of the winds might with equal propriety be called the 'gravitation theory,' for it ascribes the circulation of the atmosphere to differences of temperature and specific gravity, which are the causes to which the circulation of the ocean is ascribed by Maury's theory.

[2] Published with the Lecture delivered on November 3.

[3] Frontispiece to this Lecture.

As regards the currents analogous to the Trade Winds: a flow of water to the equator in the under strata has been clearly proved by the recent explorations. In the central parts of the Pacific the temperature of the bottom stratum of the water at the equator shows that it comes from a colder region. Towards the west of the Pacific Ocean this chart[1] shows the existence of ridges of shallow water on each side of the equator; and, in fact, between these ridges the water is found comparatively warm even at the bottom of the ocean; for, as is well shown by the 'Challenger' Reports, the flow of cold water to the equator is there checked by the ridges. In fact, the westward pressure along the surface of the ocean fills the western part of the equatorial regions with the warm surface-water checked by the land in its westward course, and which is thus accumulated in the hollows protected from the pressure of under currents from NE. and SE.

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[1] A large chart, recently drawn by the Royal Geographical Society, showing the depths of the ocean.

If we turn to the western part of the equatorial regions of the Atlantic there we find shallow water on the north but not on the south side of the equator, and in fact water little above freezing-point is found in that part of the ocean, which doubtless comes from the south, as stated by the 'Challenger' Reports; for the barrier on the north checks the deep underflow from that side. It is, however, to be observed that neither in the Section across the North Atlantic from the West Indies to the coast of Africa, nor in that across the South Atlantic from the coast of Brazil to the Cape of Good Hope, has water been found so cold as that under the equator. This does not appear to me at all surprising, because, just as the Trade Winds form an accumulation of air in the calm belt where they meet to rise upwards, so also under the equator there would naturally be formed an accumulation of the cold water, whether poured in from north or south; and the thermometer could scarcely miss finding the cold water thus accumulated about the equator; whereas farther south that cold water doubtless streams through the deepest, and perhaps narrow, channels at the bottom of the ocean; and the thermometer might be dropped many times without chancing to fall exactly in any one of such channels. The nearest point at which water so cold as that under the equator has yet been found is to the eastward of the River Plate.

As regards the oceanic under-currents analogous to the Anti-Trade Winds: according to this Section the water which sinks in the temperate zone should flow northwards along the bottom of the ocean, being more especially accumulated on the eastern side of the ocean. We find, however, that in fact, just as the barriers about the equatorial regions in some places check the flow of cold water to the equator, so also a barrier stretching from the North of Scotland towards Iceland checks the flow of the comparatively warm water along the bottom of the ocean in that direction.

Along the north side of that barrier, and below its level, there is a sudden fall of temperature; but above the level of the barrier, and all along the coast of Norway at the bottom of the ocean, the warm water is found. And about Spitzbergen, and probably much farther north and east, it forms a vast accumulation of warm water at the bottom of the ocean where it rises, and then, as a cold surface-drift, flows south-westwards. To the NW. of Spitzbergen, at the edge of the ice-fields, where the surface of the water is at freezing-point, the temperature rises at the bottom to 38° or 42°. That warm water gets chilled as it rises to the surface to flow SW. And it will doubtless be found that it is the cold surface-water thus packed against the North of Greenland which 'banks down' against that coast, and is thus packed along the northern side of the barrier which extends north-westwards from the North of Scotland.

If there be any deep channel extending eastwards from the Faroe Islands, then the cold water which rises to the surface along the eastern shores of England and Scotland will probably prove to be water which last saw daylight at the surface of the ocean in the Paleocrystic Sea; and perhaps imparts to the air of that part of our shores which it reaches properties brought from those regions of the far North by its submarine course, just as the warm surface-flow from the West affects the climate of the Scilly Islands and South-west of England and Ireland.

It is evident that, according to these Sections, the cold bottom-water could not pass from the North of Iceland into the North Sea but for the existence of the barrier which shelters it from the pressure of the warm water from the South.

In the North Pacific the rising of comparatively warm water along the shores of the north-eastern part of the ocean and its accumulation in the north-west as extremely cold water, having been chilled whilst traversing the surface in the far North, just as the warm water which rises beyond Spitzbergen is chilled on the surface as it flows towards the Paleocrystic Sea, was made very apparent by the researches of the United States Exploring Expedition in the 'Tuscarora.'

The summary of the results of that expedition, as regards the circulation in the North Pacific, I was at first inclined to regard as a hoax, compiled from a study of these Sections, and not from facts, because they are so much more closely in accordance with these Sections than I expected the actual circulation of the ocean could prove to be. For I expected that the circulation which the conflicting action of gravitation tends to cause would be greatly deranged by the action of other causes for circulation. I have, however, gradually been forced to recognise the fact that the circulation shown by these Sections describes the actually existing circulation. I was mistaken in supposing that this circulation would be greatly deranged by the action of evaporation and precipitation, for the following reasons.

As fast as the level of the ocean in any part is reduced by the removal of the vast volumes of water which are raised from the surface by the action of evaporation for the formation of the rain-clouds, the level is instantaneously restored by the action of gravitation extending over the whole mass of the ocean, and not (excepting in shallow water or narrow channels) by the formation of a current; and, therefore, the circulation flows on unchecked, whilst the level is being constantly deranged and restored. And then, where the action of precipitation brings down vast quantities of water on other parts of the wide and open ocean, not only is the level instantaneously restored by the action just described, but also the rain, even before it reaches the ocean, is under the influence of the circulating forces resulting from the conflicting action of gravitation; and, therefore, the water immediately falls into the course of circulation determined by that conflicting action.

The action of evaporation and precipitation has, however, a further effect than the tendency to cause a changing of the level of the ocean; for where there is evaporation, there the remaining surface-water is rendered more salt and dense; and, on the other hand, where there is precipitation, there the surface-water is less salt and dense than the average of sea-water. As regards the effect of the differences of specific gravity thus caused, it might perhaps be considered sufficient for me to point out that Sir Wyville Thomson, in one of the most recent of his Reports to the Admiralty, states that he has

> 'never seen, whether in the Atlantic, the Southern Sea, or the Pacific, the slightest ground for supposing that such a thing exists as a general vertical circulation of the water of the ocean depending upon differences of specific gravity.'

I cannot, however, believe that Sir Wyville Thomson intended by this to assert that there is not, in any part of the ocean, any appearance of the vertical circulation being in accordance with the course which the existing differences of specific gravity tend to cause. Because, for instance, to the south-east of the Cape of Good Hope we find the warm water from the Indian Ocean sinking below the cold water from the Antarctic regions; whereas off the coast of the United States we find the cold water from the Arctic regions sinking under the warm water from the equatorial regions. If, therefore, in the one case the course of the circulation does not appear to accord with the difference of specific gravity, it must be found to do so in the other case. In fact, there are localities in which it appears to me that the vertical distribution of the currents is not directly at variance with the idea of its resulting from their difference in specific gravity ; and therefore I cannot suppose that Sir Wyville Thomson intends to be understood to mean to say more than that a general view of the circulation in each ocean shows that the circulation is not due to the differences of specific gravity. Those who were present at the discussion which followed the Lecture I gave on Nov. 3 will remember the interesting manner in which Dr. Rae ascribed the rising of the warm water in the Arctic regions to differences of specific gravity. I will not contend that specific gravity has not anything to do with that rising of the water in the Arctic regions; it is sufficient that not only does the rising of the water there accord with these Sections, but also the course of the warm Agulhas current, sinking eastwards in the temperate zone, and therefore passing under the cold polar water which lies east of it on the surface; and the course of the cold Arctic water, sinking eastwards in the temperate zone, and therefore passing under the warm Gulf Stream water which lies east of it on the surface of the ocean—these all accord with the circulating action described in these Sections.

I was not aware, until after reading the report of the discussion which followed the Lecture on Nov. 3, that Dr. Rae had expressed the opinion that off the coast of the United States the warm Gulf Stream water passes under the cold Arctic water; and I think this must have resulted from momentary forgetfulness on Dr. Rae's part, as the reverse is well known to be the case. The general course of the lines of equal temperature there inclines eastwards from the surface of the ocean downwards. It is farther north, where the centrifugal force of astral gravitation is drawing the surface-water away from the polar regions, that water from the temperate zone is drawn northwards along the bottom of the ocean.

It is not my purpose in this Lecture to discuss the action of differences of temperature, but merely to point out that the existing circulation accords with the conflicting action of gravitation, as described in these Sections.[1]

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[1] The Second Edition of the first part of this Lecture contains a diagram of river currents, which Dr. Rae was good enough to supply for the illustration of his views regarding the circulating

Let us now turn to the Antarctic regions. The highest southern latitude reached by the 'Challenger'

action of differences of specific gravity, and differences of temperature. It is remarkable that the action of these forces has nothing to do with the circulation in the illustration: and also that I have, in papers published in 1871,* shown that in the motion of a river the same action of gravitation, controlled by the nature and configuration of the ground over which the water flows, can be traced as that under which I have described the circulation of the ocean.

Dr. Rae's candid criticism and his great practical experience induce me to venture to hope that he may, perhaps, be led to perceive that the force of gravitation which moves the water in his diagram is also causing a general circulation in the ocean, and acting with little more regard for the differences of temperature and specific gravity of the ocean-water, than for those of the river-water illustrated by Dr. Rae's diagram.

Regarding the specific gravity of the surface-water of the ocean, it must be borne in mind that the longer the surface-water is exposed to the evaporating action of the winds, the more salt and dense it must become; and, therefore, if the water is rising at the equator and at the poles, and sinking along the temperate zones, it is in these latter regions that the surface-water should be found most dense. To attribute the sinking of the ocean-water there to the greater density of the surface-water in those regions, is to invert cause and effect; for a greater density of the surface-water along the zones where the ocean is drawn downwards by the conflicting action of gravitation, is an effect which that circulating action naturally tends to cause.

A mistake, similar to that just alluded to, is constantly made regarding the circulation of the atmosphere. For by some it is asserted that the winds blow from the tropics to the equator, because the barometric pressure is less along that region; whilst others assert that the barometric pressure is less along the equator, because the air is streaming upwards there. These assertions are almost equally erroneous, for the phenomena are both effects of the same cause. It is because, in the conflicting action of gravitation, astral was 65° 42′. There, with the temperature of the air 33°, the surface-water was 29·5°; 100 fathoms below the surface, 29°:—

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* *The Standard*, Buenos Ayres, May 20, May 30, and June 4, 1871.

> 'The six thermometers used at the greater depths of 300, 500, and 1,675 fathoms, each registered a decidedly higher maximum temperature, showing distinctly that they had entered or passed through a warmer stratum of water than those used between the surface and a depth of 200 fathoms.'[1]

This statement clearly accords with these Sections, for it indicates the cold surface-flow from the South. The immediate surface is warmed up to 29·5° by contact with the warmer air; but immediately below, there is found the cold temperature of 29°, brought from the far South. The bottom-water is warmer than the surface, because it is the water which has sunk down from the surface of the ocean in the temperate zone, and therefore carries with it along the bottom of the ocean in some measure the temperature of that warmer climate. The under-current of the ocean is warmer than the surface, for the same reason that the air is warmer. The surface of the ocean flows from the far South, whilst the under-current below it, and the Anti-Trade Wind above it, flow from the

gravitation predominates along the equator, and terrestrial gravitation along the tropics; that the air is, therefore, drawn downwards and the pressure increased in the latter regions, whilst the air is drawn upwards and the pressure diminished in the former region.

[1] Report to the Admiralty by Captain G. S. Nares, dated July 30, 1874.

temperate zone towards the pole. For the reasons already given the warm under current in the ocean will be found more constant than its counterpart, the Anti-Trade of the atmosphere.

Dr. Carpenter's theory that the cold of the Antarctic regions causes the surface-water to sink in those regions, and thus causes a circulation along the surface from the equator to the frozen regions and back in the under strata, may be very good as a theory; but that it is not the cause of the existing circulation is rendered evident by the clearly-established fact that the surface of the ocean flows northwards from the Antarctic regions to the temperate zone.

This northward motion of the surface-water from the Antarctic regions places Dr. Carpenter's theory in precisely the same predicament as the theory of the currents being due to the action of the winds is placed in that locality. For suppose the convergence of the surface-currents to the temperate zones, and the sinking of the water there, were ascribed to the action of the winds blowing the surface-water from the pole and from the equator to the temperate zone, and thus giving the water accumulated there in this manner a tendency to sink downwards; this might be accepted as a very reasonable explanation in the absence of a knowledge of the fact that the winds do not converge towards the temperate zone, but blow away from it towards the pole and towards the equator. It then becomes evident that that course of the surface-water is not determined by the action of the winds, but by the action of some force which carries it along its course in spite of the opposing action of the winds.

Over a great part of the ocean the surface-water flows with the wind, because, in the circulation caused by the conflicting action of gravitation, the ocean and the atmosphere are together carried westwards in the equatorial regions, and eastwards in the temperate zones; and on the east of the temperate zone in each ocean the surface-water, as it pours against the continent, is turned aside towards the equator and the pole, and thus given, there also, the same course as the winds which blow over it. But in the central parts of the ocean, where neither the winds nor the currents are affected by the coast-lines, there the surface-water converges towards the temperate zone, whilst the winds diverge, just as indicated by these Sections. This seems very clearly to show that the main features of the circulation of the ocean are not due to the winds, but that there is a stronger force than that of the winds causing a circulation. The surface-water of a river may sometimes be seen blown backwards by the action of the winds; but in other cases the force of gravitation which draws the water onwards is greater than the force of the wind; and then the lightest straws, floating quite on the surface of the water, may be seen borne onwards against the wind. And, that the latter of these instances, and not the former, is applicable to the ocean, is made evident by the fact that though the Trade Wind blows over a great part of the Sargasso Sea and sweeps over the surface of the ocean to the shores of Brazil, the great mass of the floating seaweed is not carried along with the wind, but follows the surface-circulation of the ocean, which is clearly independent of the winds. If the winds were the cause of the existing circulation of the ocean the surface-water would drift with the wind in the Trade Wind region, and the Sargasso weed would then be swept to the northern shores of Brazil and into the Caribbean Sea.

I wish, before proceeding further with the question of oceanic circulation, to allude to an important subject of deep-sea exploration, regarding which 'common sense' has quite recently changed sides in a very determined manner. I am the more willing to offer another illustration of the fickleness of 'common sense,' because it enables me to pay a tribute of admiration to the work of a man who is older than I, and who was an enthusiastic advocate of deep-sea exploration before I knew anything of the subject.

In the year 1868 we were all startled by the wonderful announcement that during the explorations conducted by Dr. Carpenter and Professor Thomson (now Sir Wyville Thomson), on board H.M.S. 'Porcupine,' 2½ cwt. of 'living sarcode' (or, in plain English, living flesh) had been brought up by the dredge from the bottom of the Atlantic.

And subsequent announcements made it appear that that mass of sarcode had, in fact, been torn away from one vast living mass which lay stretched along the bottom of the Atlantic; all round the Great Southern Ocean; branching into the Indian Ocean; through the South Pacific; and into the vast depths of the North Pacific Ocean.

The announcements were made under such high authority and with such a weight of circumstantial evidence, that I think it was really as a mere matter of 'common sense' that the great majority of us believed the profound depth of the ocean to be paved throughout its vast extent with one unbroken mass of living flesh, which was recognised by the Royal Society as 'Bathybius.' There chanced, however, to be one among us who had made a special study of the question of life in the deep sea, and he immediately commenced an uncompromising attack upon this monstrous mass. The discussion lasted vigorously for six years, at the end of which time Bathybius was acknowledged to be dead. 'Common sense' completely changed sides on that question, and resolved the supposed living pavement of the ocean to be really as dead as the paving-stones of the streets of London. The gentleman who made 'common sense' change sides on that question is Dr. Wallich, the pioneer of the deep-sea explorations conducted by the British Navy. But for the superior knowledge of the subject possessed by Dr. Wallich, and the genuine common sense which he brought to bear upon it, we should probably all of us now still be under the influence of the strange delusion propagated by the leaders of the 'Porcupine,' 'Lightning,' and 'Challenger' expeditions.

I cannot quit this subject without alluding to a paragraph bearing upon it, written by one of the most renowned of modern philosophers. Professor Huxley, in the 'Contemporary Review' for May 1873, says: 'It is obvious that the "Challenger" has the privilege of opening a new chapter in the history of the living world.'

In writing that paragraph Professor Huxley made a double mistake. In the first place, he forgot that the chapter had already been opened by Dr. Wallich, and opened with an amount of ability, clearness, and precision worthy of the splendid advance which it constitutes in our knowledge of the world.[1] And, secondly, he was mistaken in supposing that the 'Challenger' Biologists were adding, or were about to add, any genuine discovery of im-

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[1] *Works, Pamphlets, Papers, &c., on subjects more or less directly relating to Oceanic Exploration, by Surgeon-Major G. C. Wallich, M.D., F.R.G.S., &c.*

April 1858.—'On Microscopic Objects.'—*Quart. Journal Microscop. Science.*

July 1858.—'On Apparatus for Dredging in the Deep Sea.'—*Quart. Journal Microscop. Science.*

Dec. 1859.—'On Siliceous Organisms found in the Digestive Cavities of *Salpæ.*' With Plate.—*Quart. Journal Microscop. Science.*

Jan. 1860.—'On the Distribution and Habits of the Pelagic and Free-floating *Diatomaceæ.*'—*Annals and Mag. Nat. Hist.*

Nov. 1860.—'Notes on the Presence of Animal Life at vast depths in the Sea.'—Pamphlet, Taylor and Francis, London.

Dec. 1860.—'On the Results of Soundings in the North Atlantic.'—*Annals and Mag. Nat. Hist.*

Jan. 1861.—'On the Nature of the Deep-sea Bed and Presence of Animal Life at vast depths in the Ocean.'—A Lecture delivered at the Royal Institution of Great Britain, Jan. 25, 1861.

July 1861.—'On some Novel Phases of Organic Life at great depths in the Sea.'—*Annals and Mag. Nat. Hist.*

Jan. 1862.—'Further Observations on some Novel Phases of Animal Life at great depths in the Sea.'—*Annals and Mag. Nat. Hist.*

Sept. 1862.—'The North Atlantic Sea-bed;' comprising the Diary of a Voyage on board H.M.S. 'Bulldog' in 1860, and Observations on the Presence of Animal Life and the formation of Organic Deposits at great depths in the Ocean.—Published by permission of the Lords Commissioners of the Admiralty. 4to. with 6 Plates. London, 1862. J. Van Voorst.

Dec. 1862.—'On the Structure and Habits of *Physalia.*'--*Annals and Mag. Nat. Hist.*

Jan. 1863.—'A Survey of the Physical Conditions of the North Atlantic Sea-bed.'—A Lecture delivered at the Royal Geographical Society, Jan. 12, 1863.

Jan. 1864.—'On the Process of Mineral Deposit in the Rhizopods and Sponges.'—*Annals and Mag. Nat. Hist.*

Jan. 1864.—'On the North Atlantic Sea-bed and its Inhabitants.'—*Quart. Journal of Science.*

May 1865.—'On the Structure and Affinities of the *Polycystina.*'—*Quart. Journal Microscop. Science.*

Jan. 1869.—'On the Vital Functions of the Deep-sea *Protozoa.*'—*Monthly Microscop. Journal.*

Feb. 1869.—'On the *Thalassicollidæ.*'—*Annals and Mag. Nat. Hist.*

Feb. 1869.—'On some undescribed Testaceous Rhizopods from the North Atlantic,' with Plate.—*Monthly Microscop. Journal.*

April 1869.—'On the Rhizopods as embodying the primordial type of Animal Life.'—*Monthly Microscop. Journal.*

portance to that which had already been made by Dr. Wallich. They have confirmed Dr. Wallich's

Nov. 1875.—'On the true Nature of the so-called "*Bathybius*."' —*Annals and Mag. Nat. Hist.*

July 1876.—'On Deep-sea Researches.'—*Annals and Mag. Nat. Hist.*

Oct. 1876.—'On some Observations by Mr. Gwyn Jeffreys on the Cruise of H.M.S. "Valorous."'—*Annals and Mag. Nat. Hist.*

April 1877.—'On the *Coccosphere*,' with Plate.—*Annals and Mag. Nat. Hist.*

June 1877.—'On *Rupertia stabilis*, a new Sessile Foraminifer from the North Atlantic,' with Plate.—*Annals and Mag. Nat. Hist.*

'In 1860 Dr. Wallich carried out an important series of deep-sea researches in connection with the soundings made on board H.M.S. "Bulldog," and obtained some very interesting results. He not only confirmed the presence of Globigerine *mud* in the great depths of the Atlantic, but also gave much evidence in favour of the Globigerinæ living at those depths. Dr. Wallich also brought up two living Crustaceæ from a depth of 2,760 feet, and living Serpulæ, Spirorbes, and Polyzoa from a depth of 4,080 feet. His most remarkable discovery, however, was that of living Starfishes at a depth of 7,560 feet. Dr. Wallich's researches mark another epoch in the history of deep-sea explorations.'—*Anniversary Address of the President of the Geological Society*, Professor Prestwich, F.R.S., now of Oxford, Feb. 17, 1871.

In a later part of the address Professor Prestwich distinctly fixes the date at which the existence of animal life at great depths in the ocean became a demonstrated fact, through the researches carried on in H.M.S. 'Bulldog,' by adding: 'We were ignorant only twelve years ago of what was to be found in the unexplored depths of the ocean' (*loc. cit.*, p. 48).

The above expression of opinion in 1871 is a confirmation of what was said to the same effect by Sir Leopold McClintock, as recorded in the 'Proceedings of the Royal Geographical Society' for January 28, 1861; and by Sir Roderick Murchison, as President of that Society, on May 25, 1863.

The opinions expressed on the latter occasion bear still more important testimony to the value of Dr. Wallich's researches, but are too lengthy for reproduction here.

work. They have shown that it is genuine, sterling work which could stand the test of subsequent investigation; and where they went beyond him (and they did in a most astounding manner go beyond him) he has shown that they were mistaken.

Though we are all of us liable to be mistaken at times, and are also, I think, all inclined to allow a mistake when frankly acknowledged and as far as possible repaired, to be forgiven and forgotten, there are nevertheless several reasons why Bathybius ought not just yet to be forgotten.

In the first place, the excuse made for the mistake is really worse than the mistake itself. The blame has, in a somewhat flippant manner, been thrown on the microscope; but if the microscope could speak it would bring an action against its accusers for defamation of character, and would win the suit in any court of justice. Secondly, if Dr. Carpenter and Sir Wyville Thomson had not so strangely mistaken sloppy mud for living flesh, they would not have had anything of importance to announce except that they had found the announcements previously made by Dr. Wallich, and of which they had themselves been the chief opponents, to be perfectly true. Instead of this, Dr. Wallich's discoveries were completely buried under the enormous mass of Bathybius, and are only now just commencing to emerge from beneath its ruins. In the course of time, and I trust in a very short time, these exploring expeditions will appear in their true light, and then the 'Porcupine,' 'Lightning,' and 'Challenger' expeditions will appear as what they really are; and they really are splendid buttresses which will for ever firmly support the great discovery made in the 'Bulldog' expedition by Her Majesty's volunteer explorer, Dr. Wallich.

A third reason why Bathybius ought not just yet to be forgotten is because its inventors are now, in the question of oceanic circulation, bringing forth a new Bathybius, as those who will follow me a little farther in the question of ocean currents will, I think, clearly perceive.

Anyone who once admits the idea of the circulation of the ocean or atmosphere being affected by the earth's axial rotation must naturally be led to consider what effect may be caused by the onward motion of the earth.

Influenced by this consideration, I pointed out, about twelve years ago, that the force of the earth's gravitation must be less at the surface of the central parts of the ocean than along the shores; and that therefore the conflicting action of astral and terrestrial gravitation must tend to carry the surface-water through the central parts of the ocean in the opposite direction to that of the earth's motion, and at the same time to form counter-currents along the shores.

I took considerable trouble to find whether or not recorded observations gave indications of any such circulation; and this resulted in my pointing out that such a circulation was, in fact, clearly indicated by a general tendency of the water to pour northwards from the Antarctic to the Arctic regions along the surface of the deep and central parts of the ocean, with at the same time a tendency to the formation of counter-currents along the shores.

These indications were made apparent merely by slight derangements of what would otherwise be the circulation caused by the earth's rotation.

In reply to this I was requested, on very high authority, to consider that the ocean is fairly represented by a thin coat of varnish, or covering of paper, on an ordinary globe, and would naturally be firmly held to the earth's surface and carried steadily along with it; that, in fact, there could not be either the cause or the effect imagined by me. This paper and varnish theory is certainly an interesting view to take of the subject; but I prefer to face the fact that the ocean is a vast volume of fluid several miles deep, with plenty of room for the play of varied forces and for the production of a complex system of circulation.[1]

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[1] The investigations conducted by Professor Rainey at St. Thomas's Hospital, to which I alluded in my first Lecture, show that even in this thin film of water the microscope would reveal the existence of a constant circulation. And even though it be admitted that it must be very difficult, if not impossible, to show that the circulation in question is caused by a conflicting action of gravi-

Let us consider how the question stands now. Some years after the above date—I think within the last five years—an instrument called the Bathometer has been invented by Professor Siemens, which tells the depth of the ocean by measuring the force of the earth's gravitation. It absolutely measures the decrease of the force of the earth's gravitation at the surface of the deep and central parts of the ocean: thus practically proving the existence of the cause which I gave for a circulating action.

And, as regards the effects asserted to result from that cause, Sir Wyville Thomson now says:—

> 'The Atlantic must be regarded in the light of an inlet or gulf of the general ocean of the water-hemisphere, opening directly from the Southern Sea.
>
> 'That the water of the Southern Sea simply wells up into the Atlantic.
>
> 'That water at any given temperature (below 4° C.) can only occur in the Atlantic where there is a direct communication with the belt of water at the same temperature in the Southern Sea without the intervention of any continuous barrier.

tation, and not by differences of temperature, it is certainly impossible to prove that the circulation shown in Professor Rainey's experiments is not caused by the conflicting action of gravitation.

It must be borne in mind, that every movement of the gyroscope, or in the spinning of a top (with which the action of temperature has obviously nothing to do), is clearly and definitely explained by the conflicting action of gravitation; and it is a mathematical necessity, that these forces thus controlling the motions of the gyroscope, must be *tending* to cause a circulation of the water; though, as far as theory is concerned, that tendency to circulation might be neutralised by the action of other forces.

'That the temperature of the Atlantic is not sensibly affected by any cold indraught from the Arctic Sea.

'That although there is a considerable flow of surface-water through the influence of wind-currents from the Atlantic into the Southern Sea, that flow is not sufficient to balance the influx into the basin of the Atlantic.

'That it is probable that the general circulation is kept up chiefly by an excess of evaporation in the region of the North Atlantic, balancing a corresponding excess of precipitation over evaporation in the water-hemisphere.'

In thus considering that all the water below 39° (4° C.) in the North Atlantic flows from the Antarctic regions, Sir Wyville Thomson asserts the existence of a northward motion, I will not say one hundred times greater, but a thousand times greater, than that which I, twelve years ago, asserted to exist.

Even the existence of a counter-current southwards is admitted by Sir Wyville Thomson; but according to him it is a surface-flow of water moving in the opposite direction to that of the average course of the south-east Trade Wind, and caused by the winds. This counter-current and its cause I will not now discuss. But, bearing in mind that the inequality in the force of gravitation at the surface of the ocean more than ten years ago suggested as a cause for a northward flow of water into the North Atlantic, is now proved to exist; and that the northward flow is now also asserted to exist, carrying northwards a volume of water vastly exceeding that suggested by me, let us now turn to the cause to which this vast flow of water northwards is ascribed; namely, to an excess of evaporation over precipitation in the North Atlantic; or, to use Sir Wyville Thomson's words—

> 'In fact, a part of the circuit of general ocean circulation passes through the atmosphere.'

There are circumstances connected with this idea of surface-water being raised by evaporation in one hemisphere, and then carried through the upper regions of the atmosphere across the equator and precipitated in the opposite hemisphere, which make the manner in which it is now brought forward by Sir Wyville Thomson very remarkable. The late Captain Maury strongly insisted on the existence of such a circulation;[1] but the idea was then not only not accepted by the generality of scientific men, but, I think I may say, was not then considered worthy even of courteous attention; and it seems to me strange that Sir Wyville Thomson should now bring forward this theory without making any complimentary allusion to Captain Maury in connection with it.

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[1] 'The atmosphere is an engine. The South Seas themselves in all their vast intertropical extent are the boiler for it, and the northern hemisphere is its condenser.'—*Physical Geography of the Sea*, sect. 168.

'The heat which this heavy evaporation absorbs becomes latent, and, with the moisture, is carried through the upper regions of the atmosphere until it reaches our climates. Here the vapour is formed into clouds, condensed, and precipitated.'—*Ibid.*, sect. 172.

What makes the matter still more strange is that Captain Maury suggested the idea of this transfer of water through the air from one hemisphere to the other, not to account for a northward motion of water through the ocean from the South into the North Atlantic, but to account for what he asserted to be an immense excess of precipitation over evaporation in the northern hemisphere.[1]

Considering that Captain Maury had made a lifelong study of these subjects, and that Sir Wyville Thomson, after being appointed Director of the Scientific Staff of the 'Challenger' Expedition, says of himself—

> 'I pretend to no special knowledge of physics, and I should have greatly preferred confining myself to the domain of Biology, my own proper province'—

and considering that, even in that domain of biology, Sir Wyville Thomson has, in company with Dr. Carpenter and Professor Huxley, made the greatest mistake that has ever been recorded in the annals of science; in face of these circumstances it does not appear to me likely that Captain Maury will prove to be in the wrong, and Sir Wyville Thomson in the right, regarding this question of evaporation and precipitation. This may, however, prove to be the case; but even then the idea of there being such a transfer of vapour across the equator as to account for the northward motion asserted to exist, or even the idea of such a vast northward motion actually being in existence, will, I am sure, before long be clearly shown to be a truly Bathybian theory. Just as Dr. Carpenter and Sir Wyville Thomson overwhelmed Dr. Wallich's discovery of life in the deep sea by the discovery of Bathybius, so also have they overwhelmed the slight northward motion for which I contended with a flow which is in all respects quite 'Bathybian.'

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[1] 'The annual amount of rain in the north temperate zone is half as much again as that of the south temperate.'—*Physical Geography of the Sea*, sect. 170.

In propagating this theory of circulation Sir Wyville Thomson appears to have been influenced by the fact that, in the observations made by the 'Valorous' Expedition, the bottom-temperatures between Ireland and Greenland were found to be (except close to Cape Farewell) warmer than the temperatures recorded by the 'Challenger' Expedition between Bermuda and the Azores, and that in the latter section no water was found so cold as the bottom-water near the West Indies.

But, as regards the warm bottom-water found by the 'Valorous' Expedition, that water forms the oceanic counterpart of the south-west winds of this latitude. It is the warm surface-water which sinks in the temperate zone to flow along the bottom of the ocean towards the pole. And, as regards the bottom-water being found colder about the West Indies than in the latitude of Bermuda, that is no more strange than the fact of the bottom-water at the equator having been found colder than at the tropic of Capricorn; for, in each case, there is an accumulation of cold water which is probably carried to those localities through deep and narrow channels into which the thermometer has not yet chanced to fall. Sir Wyville Thomson is, I think, completely mistaken in supposing that the cold water of 34° at the bottom of the ocean near the West Indies comes from the Antarctic regions. It seems to me quite obvious that that cold water is supplied by an undercurrent flowing southwards from the banks of Newfoundland, which returns to the equatorial regions an equivalent for the water carried into the Arctic regions by the warm surface-flow northwards from the temperate zone on the east side of the ocean. Future explorations will settle this question. I have already suggested that the heating action of the sun's rays acting on the equatorial current all the way from the Gulf of Guinea to the Gulf of Mexico, in the North Atlantic, and only from the Gulf of Guinea to the coast of Brazil, in the South Atlantic, is an obvious cause for the existence of a great excess of temperature in the North Atlantic; and if that excess of temperature were not found actually to exist, that would tend to prove the existence of a free circulation between the two oceans; but the fact of a great excess of temperature being found in the North Atlantic rather tends to an opposite conclusion.

I shall now confine myself to one point in reference to the tides.

As, by the new theory of Vis-Inertiæ, the motive power which carries the earth along its orbit is gravitation, the idea naturally suggested itself to me that if this is really the true cause of the earth's onward motion in its orbit round the sun, then there ought to be a tendency to the formation of a higher tide raised on that part of the earth which is in advance in the orbital motion than on the opposite side. And I therefore took considerable trouble in endeavouring to find any observations which might show the spring-tides following the September equinox to be higher than those following the March equinox; but (as stated in my work published in 1866) no such observations existed. The discovery of such a tide has, however, since been announced. In an address delivered last year by the President of the Geographical Section of the British Association, Captain Evans, Her Majesty's Hydrographer, states that recent tidal investigations indicate that—

> 'We may have to recognise this physical condition, that the waters of the southern hemisphere attain a high level at the period of the year when the sun is to the north of the equator, and that the northern waters are highest at the period when the sun is to the south of the equator.'

Captain Evans suggests that these tidal differences are not due to the action of the wind, nor to any merely meteorological, but to some cosmical cause.

The high posts which have been held by Captain Evans, as Hydrographer to Her Majesty, member of the Council of the Royal Society and of the Council of the Royal Geographical Society, carry with them high responsibilities. And, considering the trouble I have taken year after year to induce him to give some attention to the subject, and that he now tells me that he has not been influenced by my writings, and did not know that I had suggested the existence of such an annual tide, I think it is his duty to state what is the cosmical cause to which he alludes, if not that indicated by the theory of Vis-Inertiæ.

Captain Evans is certainly mistaken on one point—that is, in his idea that he has in this matter opened up 'new fields for observation and research.' They are fields which had been restlessly trampled by me ten years before Captain Evans read his paper. I will not say that it was Captain Evans' duty to be acquainted with what I have written on the subject, but it certainly was his duty to be better acquainted with the literature of the subject than to present to the public as a virgin field for research that which had, in fact, long before been well trampled by me.[1]

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[1] The cause for this annual tide was given in a Paper which was rejected by the Royal Society in 1868, whilst Captain Evans was a member of the Council of that Society. If the tidal differences pointed out by Captain Evans really result from the action of this annual tide, then the morning tides should show an exactly similar excess over the evening tides in both hemispheres, but this could not be made evident without an extensive series of observa-

I sought for that annual tide in consequence of having attained what I considered certain evidence that the earth's onward motion is caused by the force of gravitation, though I could not then imagine where the source of that power could be. When I afterwards discovered that that power is the revolving force of the sun's gravitation, which is inversely as the cube of the distance—not from the nearest part of the sun, nor from the centre of the sun, but from some other point below the surface of the sun—I pointed out that the squares of the periods in which the planets are revolved in their respective orbits round the sun actually represent the relative amounts of the revolving force of gravitation in each orbit. I also pointed out that this revolving force requires a different relative velocity from that indicated by Kepler's Law; though it appeared that at the actual distances of the planets the difference was not sufficient to be practically apparent; but, as that difference increases at a very great ratio as the sun is approached, I have always maintained that if a planet should be discovered much nearer to the sun than Mercury, then the difference would certainly be made clearly apparent by the new planet having too fast

tions; and these observations ought to make practically apparent the error of supposing that the tides which rise against the western shores of Europe are the tides raised a day and a half before in the Great Southern Ocean, which was, in fact, shown to be a mistake in the Paper above alluded to.

a motion to allow of Kepler's Law being considered applicable to it, as that law has been considered applicable to the relative velocities of all the planets at present known.

I was led to consider that the error in Kepler's Law is not apparent in the motions of the known planets, because I found that every astronomical work which mentioned the subject asserted that the velocities accord with Kepler's Law, except that a correction has to be made in proportion to the size of each planet. I accepted these assertions without testing them by figures, and supposed that the question could not be brought to a practical test without the discovery of an inter-Mercurial planet. But a few weeks ago a friend kindly sent me a copy of Professor Clerk Maxwell's text-book on 'Matter and Motion.' There I saw the old assertion regarding Kepler's Law repeated; but, besides this, Professor Maxwell also gives a table of figures showing what the discrepancy actually is. The figures show that the correction to be made does not vary in proportion to the size of the planets, but does vary as their distance varies, and as is required by the theory of Vis-Inertiæ. It is, therefore, now no longer requisite to wait for the discovery of another planet to give the further test of the correctness of the theory of Vis-Inertiæ, which has, in fact, given the reason for the actual relative velocities of the planets, for which no clear and definite reason has ever before been given since the world began. And any impartial student of the subject must become convinced that the reason and law thus given must stand good as long as the world endures; for the onward motion of every planet, and not only of every planet, but also of every satellite in the heavens, confirms its truth. Besides all this, it must be borne in mind that if two stones of different sizes be allowed at the same moment to fall from any height—that is to say, if they be left free to move under the action of the earth's gravitation—they are then by that force of gravitation carried along to the ground at exactly the same rate. And, therefore, if the onward motions of the planets be really caused by the action of the sun's gravitation, as indicated by the theory of Vis-Inertiæ, then, as that force of gravitation which carries them along tends to carry them all along together, without regard to their different sizes, the differences in their relative velocities must depend on those of their relative distances, and not on their relative sizes. Therefore, in the actual velocities of the planets there is a double confirmation of the theory of Vis-Inertiæ.

The illustration I have given of these two stones lifted up and then allowed to fall at the same moment under the action of the earth's gravitation, is applicable to the motions of the planets, inasmuch as the latter are all being carried along at the same time by the revolving force of the sun's gravitation. But if, instead of taking up these stones and dropping them at the same moment, I take them up and drop them singly, then the case is altered ; and though practically we could not distinguish the difference in the velocity with which the stones fall, still, mathematically speaking, the velocity of the small stone must be greater than that of the large stone. For, as indicated at the commencement of this Lecture, on Nov. the 3rd, it is clear that if this stone were sufficiently increased in size, and the force which now holds it and the earth apart withdrawn, then, instead of the stone moving to the earth, the earth would move to the stone, and the motion of the stone would be practically imperceptible. If, instead of being made so very large, the stone were made only the same size as the earth, and the force holding them apart withdrawn, then the stone and the earth would each not only have one-half of the motion caused by their mutual attraction, but also would each move through half the space by which they were previously separated.

Thus, if dropped at intervals, the motions of these stones, as moved by the earth's gravitation, would not be identical; though when dropped together they are so ; and as the planets are all being moved at the same time by the revolving force of the sun's gravitation, there is not the correction to be made for their different sizes which would have to be made for two stones of different sizes if dropped at different times.

The existence of this revolving force of gravitation, which in theory is established as an irrefutable deduction from Newton's laws of gravitation, is also practically established by the manner in which its action is corroborated by the motions of the planets. And, this revolving force being the cause of the onward motions of the planets, then the idea of those motions resulting from what has been termed a 'primary push or impulse,' and an innate tendency to move for ever in straight lines under the action of that impulse, must be discarded.

The assertion that a body in motion has an innate tendency to move for ever onwards in a straight line has been termed an 'Axiom, or Law of Motion.' It is not an axiom at all, but a complex proposition. Newton's excuse for putting it among the axioms, instead of among the propositions to be proved by argument, was, that it could not possibly be proved, but had to be admitted to explain what appeared otherwise inexplicable. It does not follow that because a proposition cannot be proved to be true it cannot be proved to be false. And this I claim to have done. I have proved it false, by showing the new theory of Vis-Inertiæ, which is incompatible with that so-called axiom, to be true. Newton did not invent this pseudo-axiom, he adopted it from Descartes ; and all the great work done by him stands good without it. One of the most interesting points in connection with this question is the fact that

Newton and Descartes have been exactly in the predicament of the two disputants well known in fable, who, when their common error (which consisted in supposing that they were each looking at the same side of the shield) was removed, found that their opinions coincided. Just so the rejection of the erroneous axiom accepted by Descartes and by Newton allows of an extension of Newton's laws of gravitation which then themselves supply the action for the explanation of which the erroneous axiom was invented. And not only so, but also that same extension of those laws shows that Newton's force of gravitation actually forms the Cartesian Vortices with which it was before supposed to be incompatible; and it shows that Descartes was also right when, in opposition to the views afterwards maintained by Halley, he ascribed to the centrifugal force of the earth's rotation a tendency to cause the Trade Winds and a general circulation of the atmosphere; though he did not know that that centrifugal force to which he ascribed the winds is, in fact, a part of the conflicting action of the Newtonian force of gravitation, as has been proved by the new theory of Vis-Inertiæ.[1]

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[1] When I delivered this Lecture, I was not aware that Kepler, as well as Descartes, was of opinion that the planets are carried onwards in their orbits by the action of a whirling vortex; and that the action of that force makes the squares of the periods in which the planets are revolved, as the cubes of their distances from the sun.

The extension I have given to the Newtonian laws of gravitation proves that, in the opinions above mentioned, Kepler and Descartes

The Cartesian and Newtonian fiction of Vis-Inertiæ tending to keep a body in motion must be abandoned—it is of no avail to merely drop the term and re-dress the old idea in newly-coined words, as has been freely done during the past ten years—for the idea (in whatever words it may be dressed) is a fiction which has served its purpose, and may now be relegated to repose in the pages of history with such other kindred fictions as the Ptolemaic and the Tychonic systems of the world, which, like it, have served as stepping-stones—as useful stepping-stones—to higher states of knowledge.[1] (Much applause.)

were right. The rejection of those opinions during the past two centuries, under the supposition of their being at variance with the Newtonian laws of gravitation, has been a mistake.

[1] Since delivering the First Part of this Lecture, I have given a good deal of attention to the action of the Gyroscope, and find in all its motions a clear corroboration of the conflicting action of gravitation there described as acting in a glass of water in exactly the same manner as it forms Newton's centripetal and centrifugal forces acting on the planets. Every movement of the Gyroscope distinctly accords with the new theory of Vis-Inertiæ (or the conflicting action of gravitation); and where gravitation acts as Newton's centrifugal force, it is easy to trace the action of this force after having arrived at a solution of the motions by the assistance of the theory of astral gravitation. This can be done in reference to phenomena which I will venture to say have never before been definitely explained, and perhaps have even never before been observed.

Some other phenomena, which are explained in a clear and simple manner by the action of astral gravitation, appear to me even after having arrived at the solution as above, quite inexplicable without accepting the *new*, and rejecting the *old*, theory of Vis-Inertiæ.

Sir John Herschel refers his readers* to a Paper by Professor

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* *Outlines of Astronomy*, Section 245 j.

I am afraid I have been somewhat hurried in what I have had to say; but, nevertheless, I hope I have

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Powell in the Proceedings of the Royal Astronomical Society for April 1855 for a lucid explanation of one of the phenomena of the Gyroscope. But Professor Powell makes the mistake of assuming that the disk of the Gyroscope is 'wholly free from the influence of gravitation,' and therefore is obliged to unnecessarily complicate the explanation of the phenomenon.

Sir John Herschel, when himself illustrating the subject by the manner in which a rotating globe, when rolled along the ground on its brass meridian, turns aside from the straight line in which it is rolled, makes the same mistake as that made by Dr. Carpenter in reference to the 'rotation of the pendulum,' namely, that of treating the phenomenon as if it depended on the motion being communicated along a meridian. The phenomenon described by Sir John Herschel in connection with the rotating globe is identical with that which causes the cricket ball, when bowled with the requisite twist, to turn aside from a straight course at the point where it pitches; and no bowler ever thinks of changing his 'twist' to allow for the difference between the wickets being placed east and west, or north and south. The infinitesimal action of the earth's rotation is quite beside the question in this illustrative case, and has nothing to do with the phenomenon under discussion, which is quite independent of the bearing of the points of the compass, though similar phenomena are caused by the earth's rotation, and are then directly connected with the bearing of the points of the compass.

I do not think it strange that Sir John Herschel and Dr. Carpenter should have made the same mistake, for the mistakes are probably connected as cause and effect; but Sir John Herschel has perhaps, merely for the purpose of explanation, unnecessarily connected the phenomenon with the motion of the earth without really supposing it to be affected by the latter. Though the phenomenon of the 'rotation of the pendulum' referred to by Dr. Carpenter is due to the earth's rotation, it is not dependent on the motion being communicated along a meridian, as required for the purpose of his illustration.

The turning aside of the globe as described by Sir John Herschel, and the turning of the cricket ball bowled with a twist, been clear enough to enable any one present to offer any observations he may feel inclined to make on what I have said, either for the purpose of seeking further information, if on any point I have failed to make myself clearly understood, or for controverting any of the opinions I have expressed; or for pointing out any thing in favour of what I have said, or any thing which may give a different interpretation to any facts adduced in favour of the theory which I have brought forward, I shall be obliged, if any one present is disposed to do so. (Applause.)

[After a short pause]

Well, as no one seems disposed to say anything, I

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are both directly caused by the action of astral gravitation in exactly the same manner. And, though both phenomena can be also explained by the action of Newton's laws of motion, I do not believe that even this less clear and simple explanation has ever been definitely given by any one.

When Descartes explained the System of the World under the supposition of the earth being in motion, he stated that he supposed the earth to be in motion for the sake of simplifying the explanation of the phenomena; but that, as the phenomena could, in a more intricate manner, be explained under the supposition of the earth being at rest, the acceptance of his views did not necessitate the rejection of this latter orthodox opinion. I cannot say the same regarding the old and the new theories of Vis-Inertiæ, though in many phenomena the same suggestion would perfectly apply.

For instance, as regards the circulation of the ocean and atmosphere; the meridional circulation indicated by the new theory of Vis-Inertiæ can be perfectly, though less simply, explained by the action of Newton's centripetal and centrifugal forces, though that explanation never was given until I was led to it by the assistance of the new theory of Vis-Inertiæ. But the greater part of the latitudinal circulation indicated by this latter theory appears to be irreconcilably at variance with the old theory.

can only apologise for having spoken in a hurried manner and not so amply as I intended to do, and to thank you very much for the very kind attention you have given.[1] (Applause.)

Dr. WALLICH :

I am sure that all who have listened to the admirable address delivered by Mr. Jordan to-night, must appreciate the great amount of labour which he has bestowed upon the subject, and the care which he has taken to divest it of the numerous technicalities which are generally associated with scientific subjects. Mr. Jordan has alluded to certain observations of mine in terms which, I fear, are far too flattering. At the same time, I thank him most cordially for the compliment he has paid me, and I hope the day will come when the arguments which he has adduced, with regard to what I have done in the way of deep-sea exploration, may be more considerately listened to than they have heretofore been. (Hear, hear.)

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[1] 'From all sides a most flattering testimony to the real intrinsic worth of your second lecture, and its calm, *excellent* delivery. A great many of the audience (and I here simply refer to my own personal friends or professional acquaintances) decidedly preferred it to your first lecture, and insist on your being a master of elocution.' I give this extract from a letter kindly written by Dr. Altschul, to show that it was no want of clearness on my part which kept the opposition from entering the lists in open discussion. I was myself quite surprised at obtaining unsought praise for the manner in which I spoke; but am proud of being able to avail myself of this opportunity to thank Dr. Altschul for any praise that may be due, for the credit is really more due to that renowned Professor than to me for any elocutionary merit in my public challenge.

It has been very aptly remarked by a writer—I forget who—that all new discoveries are invariably regarded as insults to the age in which they are made, on account of their convicting it of ignorance. I am afraid that, in our own day, this saying conveys nothing but the truth. There happen to be at the head of the scientific world in the metropolis several men who are deservedly held in high repute, and have done a great deal of good whilst gaining that reputation—but who build rather too much upon it, and are far too much inclined to pooh-pooh the investigations of those who do not already belong to their own party. In Mr. Jordan's case, I think we have a marked example of this kind of treatment at the hands of leaders in science. As I mentioned on the last occasion when Mr. Jordan lectured on this subject, he has done his work single-handed—a great portion of it on the other side of the Atlantic—away from libraries and other sources of information, working simply for the sake of eliciting truth, therefore I leave it to you, ladies and gentlemen, to decide for yourselves after what you have heard, and the admirable way in which he has put forward his theories, whether he does not deserve some better treatment than he has received from those who affect to think that no new hypothesis or discovery is deserving of notice, unless it be sent forth with their *imprimatur*. (Hear, hear.) Mr.

Jordan has, I think, proved that the time has come when a different order of things ought to be inaugurated. Surely men who have devoted the greater part of their lives to the investigation of great physical phenomena (even supposing that their conclusions may, in certain instances, demand revision) ought to receive impartial consideration from those whose position at the head of societies supported by the State makes it their duty to be acquainted with the progress of scientific investigation. (Applause.) I am sure you will all most cordially join with me in a hearty vote of thanks to Mr. Jordan, for the address to which we have this evening had the pleasure of listening. (Much applause.)

CAPTAIN MARSHALL HALL seconded the resolution, which was carried unanimously.

Mr. JORDAN:

Ladies and Gentlemen,—I thank you again for the very kind attention you have given me; and though in face of the circumstances under which I have given these Lectures, publicly challenging discussion, I have no right to shelter myself with any apology or excuse for shortcomings, still I think I may state, as a mere matter of fact, that I had prepared a list of notes and references to guide me through the subjects I had to speak about, but by accident I have not had them; and therefore I must give that as an apology for any fault there may be found in the chain of argument, and, on this account, I the more regret that those I have invited to the discussion still avoid it; for in the course of discussion, any omission I may inadvertently have made would have been supplied; but still I hope that I have been able to make myself intelligible in what I have said. (Much applause.)

APPENDIX.

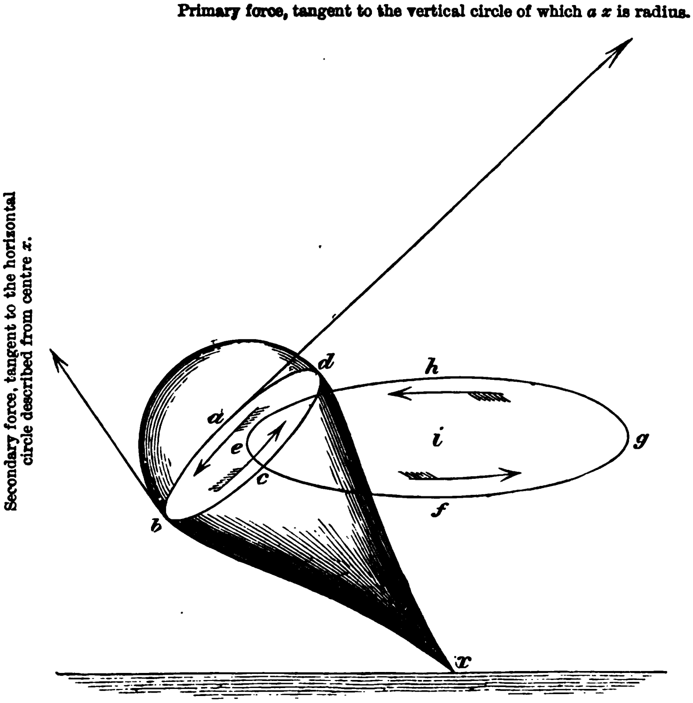
A Gyroscope which I have had specially constructed shows that it is not the motion of the rotating disk, but the force which resists that motion, which endeavours to keep it in the plane of rotation.

This forms a practical and clearly apparent demonstration of the correctness of the new theory of Vis-Inertiæ, according to which 'I have shown that the *motion* imparted by a rotating surface cannot act as a centrifugal force; but that the latter is a force which *resists and retards the motion.*' ('The Winds,' Preface, p. xiii.)

The Gyroscope makes evident the fact I have been contending for during the past twelve years, namely, that it is not the onward motion of the earth in its orbit, but the force which resists that onward motion, that forms the centrifugal force which opposes the centripetal force of the sun's gravitation. This has, however, been already superabundantly shown in other phenomena.

The following diagram further illustrates the manner in which the force of astral gravitation, as described in the First Part of this Lecture, supports the top by its action on the side *a*, and not on the side *c*.

The tangential action of the force at *a*, which


Primary force, tangent to the vertical circle of which *a x* is radius.
Secondary force, tangent to the horizontal circle described from centre *x*.
d
h
a
e
c
i
g
f
b
x

supports the top (constantly lifting that side whilst the side *c* falls) carries the top round with the motion of revolution in the direction *f*, *g*, *h*.

A force of astral gravitation at the point *b* then resists this horizontal motion exactly as that at the point *a* resists the downward motion; and the tangential action of the force at *b* also supports the top; because it tends to carry the point *b* farther than the point *d*, from the centre *x*, just as the tangential action at the point *a* makes the top revolve by tending to carry the point *a* farther than the point *c* from the centre *x*.

As soon as the velocity with which the top rotates becomes so slow that the excess of the force of astral gravitation resisting the motion on the side *a* is no longer greater than the force with which the earth's gravitation draws the top downwards, it falls under the influence of the earth's gravitation; the onward motion communicated to it by the action of astral gravitation is immediately reversed, and it runs on the ground like a wheel.

The manner in which astral gravitation, as just shown, supports the spinning top, and controls its motion, is identical with that in which it acts in circulating the ocean, the atmosphere, and the moon over the surface of the earth, and the planets over the surface of the sun; and it is corroborated by the manner in which every movement of the Gyroscope accords with its action, whereas some of the latter movements cannot be made to corroborate the Cartesian and Newtonian Laws of Motion.

THE DISCOVERY OF LIFE IN THE DEEP SEA.

My dear Mr. Jordan,—As you have acquainted me with your intention of publishing the Lecture delivered by you at Willis's Rooms on December 20, it strikes me that it would be well to state in connection with that portion of your address in which you alluded to my having in 1860 opened out a new page in the history of deep-sea exploration by the discovery of highly organised animal life at really abysmal depths in the ocean, that, as a matter of fact, several presumably living creatures had, so long previously as the year 1819, been obtained by Sir John Ross in Baffin's Bay, from a depth of about 800 fathoms; but that the discovery was so curtly and imperfectly recorded in his official journal, as to have led to its being altogether discredited until 1862, when its claims to credence were set forth by me in detail.[1]

These circumstances ought, I think, to be explicitly stated, both on your behalf and my own, inasmuch as the very writers who, within the past eight years, have ostentatiously tried to make it appear that *they* were the foremost to rescue Sir John Ross's observations on this subject from oblivion, are themselves among the number of those who had ruthlessly ignored them. On the other hand, it is pleasant to be able to cite many illustrious leaders in science who were too honourable and candid not to admit without reservation that, until the fact was clearly demonstrated by the results of the 'Bulldog' explorations in 1860, they were disbelievers in the existence of animal life in the ocean at greater depths than 300 or 400 fathoms.

Yours sincerely,
(Signed) G. C. WALLICH.

No. 162 Holland Road, Kensington:
January 24, 1878.

When I spoke on December 20, I was not aware of the facts alluded to in the above extract from a letter received from Dr. Wallich just in time for insertion in this Appendix. I feel sure that the plain, straightforward, candid tone of Dr. Wallich's statement will be appreciated by the vast majority of readers. The portion of Dr. Wallich's work referred to in the foregoing letter concludes with the following sentence:

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[1] 'The North Atlantic Sea-Bed,' pp. 79, 80.

> In attempting to reconcile these facts, I desire to do justice both to Sir James Ross, who unquestionably perceived the full force of Sir John Ross's researches, and to Mrs. Somerville, who, in defiance of the unbelief of 'naturalists and geologists,' recognised the all-pervading principle of life where its presence had been so long and so obstinately repudiated.

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